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OBSERVATIONS
ON
THE MUSCLES,

AND PARTICULARLY ON
THE EFFECTS OF THEIR OBLIQUE FIBRES:

WITH
AN APPENDIX,

IN WHICH
THE PRETENSION OF DR GILBERT BLANE,
THAT HE *FIRST* DEMONSTRATED THE SAME EFFECT TO BE
PRODUCED BY OBLIQUE MUSCLES AS BY STRAIGHT
ONES, WITH A LESS PROPORTIONAL DE-
CURTATION OF FIBRES,
IS PROVED TO BE UNFOUNDED.

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OBSERVATIONS

ON THE

MUSCLES, &c.

AS it appeared to me, when I firſt began, in 1759, to deliver in this Univerſity a public courſe of lectures on Anatomy and Surgery, that the ſtructure of the oblique muſcles had not been ſufficiently examined, nor even the number of them attended to by authors, and that ſome of their chief purpoſes or effects had been entirely overlooked by them, I endeavoured then, and in every courſe of lectures ſince that time, to direct very particularly the attention of ſtudents to thoſe ſubjects.

I BEGAN with obſerving, as a material defect in the otherwiſe very accurate and elegant tables of ALBINUS, as well as in the former ſyſtems of VESALIUS, EUSTACHIUS, BIDLOO and COUPER, that the tendinous membranes or apaneuroſes, with which many muſcles, particularly of the extremities, are covered, and with which the oblique muſcles are cloſely connected, were not delineated, yet that the knowledge of theſe is not only of uſe in the practice of ſurgery, but for underſtanding the action of the muſcles.

I REMARKED, that although in ſome parts of the body, tendinous membranes, ſuch as thoſe between the cartilages of the ribs, or the apaneuroſes palmares, or faſciæ latæ of the thighs, ſerved merely for the defence of the parts, or as ſheaths to them, as they were connected to them by the cellular ſubſtance only, yet, in general, they ſerved, beſides the mere purpoſe of defence, to furniſh a greater extent of ſurface for the attachment of oblique fleſhy fibres.

I SHEWED them, that wherever tendinous membranes run longitudinally connected to the opposite ~~on the~~ ſurfaces of muſcles, fleſhy fibres, placed obliquely, were found; that in many muſcles, as in the ſemi-membranoſus, or flexor pollicis longus, fleſhy fibres paſſed obliquely from the inner part of the tendon on one ſide, to the inner part of the tendon on the other ſide of the muſcle, or ſuch muſcles were ſemi-penniform, (See T. 1. fig. 1.); that in other muſcles, as in the rectus extenſor cruris, or flexor pollicis pedis longus, a third tendinous membrane was found in the middle of the muſcle, between which and the inner parts of the tendons on the two ſides of the muſcle, the fleſhy fibres paſſed obliquely, and produced a complete penniform appearance, (ſee T. 1. fig. 2.); and ſome muſcles, as the ſoleus, might be called compound penniform, becauſe, on cutting them lengthways, we diſcovered ſeveral longitudinal tendinous membranes, to both ſides of which oblique fleſhy fibres were connected. See T. 1. fig. 3.

I ALLEGED, that the direction, length and number of fibres in ſuch muſcles had not been ſufficiently attended to by anatomiſts or by ſurgeons; and that, in many inſtances, the breadth of theſe muſcles had been miſtaken for their length; that in conſequence of ſuch inattention, they would find the celebrated LOUIS * attempting to diſcard the double inciſion of the ſoft parts in the amputation of the thigh, although, from the obliquity and ſhortneſs of the muſcular fibres which cover the thigh-bone, this improvement of CHESELDEN is more eſſential than in the amputation of the humerus, where the fleſhy fibres, though oblique, are proportionally longer, and of courſe their retraction greater.

WHERE the fibres of muſcles run obliquely, it is evident, and has been obſerved by BORELLUS and others, that the fibres will be more numerous than if the ſame ſpace had been covered with longitudinal fibres; and although an oblique fibre will not raiſe a weight with the ſame force as a ſtraight fibre, yet the number of the fibres may be ſo much increaſed by their obliquity, as to do more than compenſate for the loſs of force occaſioned by the obliquity. Thus, let us ſuppoſe a longitudinal muſcle to be five inches long, and one inch in breadth, and let us ſuppoſe it to contain in its breadth four fibres or ropes, each one-fourth of an inch in diameter, as in T. 2. fig. 1. the force of this muſcle may be repreſented by the number 4.

LET us next ſuppoſe theſe ropes to be cut into pieces, each of which is one inch and a quarter in length, as repreſented by dotted tranſverſe lines A, B, C, we ſhall, by doing ſo, form 16 ropes or fibres.

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* Acad. Roy. de Chirurg. T. 2. p. 357.

Let us next ſuppoſe, that theſe ropes, repreſenting muſcular fibres, are laid obliquely, like the hypotenuſes of right-angled triangles, of which the baſes are equal to one inch, and the height or perpendicular equal to three quarters of an inch, as in T. 2. fig. 2. each ſuch fibre will, as Borellus has demonſtrated, loſe two-fifths of its force. But as there are 16 fibres inſtead of 4, their force will be as 16 multiplied by 3, to 5 multiplied by 4, or as 48 to 20, or as 12 to 5.

But that the mere increaſe of the number of fibres, or force of the muſcles, which alone has been obſerved by authors, is not the ſole purpoſe of nature, appears from this, that in ſome places, and particularly between the ribs, oblique fibres are employed, although it is evident, that a greater number of ſtraight fibres, or of fibres perpendicular to the ribs, might have been diſpoſed in the ſame ſpace.

The other purpoſes of oblique muſcles, and which had not been perceived by authors, are,

To perform much more extenſive motions with the ſame degree of ſhortening of the fleſhy fibres, than can be performed by ſtraight muſcles, or with a leſs degree of ſhortening of the fibres, to perform motions of equal extent.

I shall now endeavour, in the firſt place, to demonſtrate, that a pair of oblique muſcles, placed between the ſame parallels with two ſtraight muſcles, perform, with the ſame proportional contraction, more extenſive motions than the ſtraight muſcles can do.

[illegible] P. 10. Line 11. [illegible]

First, As one oblique muſcle, ſo ſituated, is longer than a ſtraight muſcle, if each be ſhortened one third, or any other proportional part, it is evident, that the place of the inſertion of the muſcle will move through a greater ſpace, when the oblique muſcle acts. Thus, in the annexed figure, (T. 2. fig. 3.) if P, L repreſent two parallel lines, and AB repreſents an oblique and AC a ſtraight muſcle, it is plain, that when each is ſhortened one third, and that the place of the inſertion is moved directly towards the place of the origin of the muſcle, the motion occaſioned by the oblique muſcle will be proportionally as much greater than that produced by the ſtraight muſcle, as the hypotenuſe, or line AB, is longer than the perpendicular line AC.

But next, let us ſuppoſe, (ſee T. 2. fig. 4.) that the point A cannot be drawn directly towards the point D or E, on account of the connections of the bones, ſuch as the ribs, which the parallel lines PA and DE repreſent; or ſuppoſe, that ſuch bones, when moving, remain parallel to each other; or let us ſuppoſe, that two oblique muſcles balance each other, ſo that their inſertions, inſtead of being moved directly towards their origins, are moved in a diagonal line, between the two muſcles.

Thus, let PA and DE (T. 2. fig. 4.) be two parallel lines, and let AB and AC repreſent two ſtraight muſcles, and AD and AE two oblique muſcles, it is plain, that when the two ſtraight muſcles have ſhortened themſelves one third part of their length, their inſertion A will be brought down to number 1. But when the oblique muſcles AD and AE, by acting together, have brought the point A down to 1, and are in the ſituation of the dotted lines 1 D and 1 E, they cannot have loſt more of their length than the length of the perpendicular A 1, which is ſhorter than the hypotenuſe A*b*, or leſs than the third of the length of the oblique muſcle. In fact, they have loſt leſs of their length than A 1, becauſe the two ſides A 1 and 1 E of the triangle A 1 E muſt be longer than the third ſide AE; and therefore oblique muſcles can perform as great a degree of motion as ſtraight muſcles, without being ſhortened in the ſame proportion; or, which is the ſame thing, if they continue to act till they are ſhortened in the ſame proportion, the place of their inſertion, A, will deſcend farther, or through a larger ſpace.

I SHALL now proceed a ſtep farther, and endeavour to demonſtrate, that where two oblique muſcles balance each other, the motion of their inſertion is more extenſive than can be produced by two ſtraight muſcles of the ſame length with the oblique muſcles.

THUS, in T. 2. fig. 5. let AB and AC repreſent two ſtraight muſcles, and AD and AE two oblique muſcles of the ſame length, and we ſhall ſuppoſe the length of each muſcle to conſiſt of any given number of inches or parts, ſuppoſe five parts, 1, 2, 3, &c. or I, II, III, &c. and when in action to be capable of ſhortening itſelf one fifth part or two fifth parts of its length. Let the two correſponding numbers 1 and I, or 2 and II, be joined by the ſtraight lines 1 I, and 2 II, ſo as to form the iſoſceles triangles A 1 I, or A 2 II. When the two ſtraight muſcles have acted fully, or ſhortened themſelves one-fifth of their length, the point A will deſcend to 1. But when the two oblique muſcles have, by their action, brought the point A down to 1, they have not loſt one-fifth of their length; for the dotted lines repreſenting them muſt be longer than the lines IE or ID, becauſe the angle 1 IE being equal to the two angles 1 IA, and 1 AI of the iſoſceles triangle A 1 I, muſt be larger than a right angle, and therefore the ſide 1 E muſt be longer than the ſide IE; that is, the oblique muſcles, after bringing the point A down to 1, have not loſt one-fifth of their length; or if they continue to act till they have loſt one-fifth of their length, they will bring the point A lower down than can be done by ſtraight muſcles, ſhortened in the ſame proportion.

To make this propoſition ſtill plainer, if poſſible, by calculation, I ſhall ſuppoſe the oblique and ſtraight muſcles in T. 2. fig. 6. to be each five parts or five inches in length; that the baſes of the triangles BD and CD meaſure four inches; and that the perpendiculars, or altitudes of the triangles, meaſure three inches; and let it be ſuppoſed, that theſe muſcles, in action, can be ſhortened one-fifth of their length, the ſtraight muſcles, on that ſuppoſition, can bring A down to 1 only: But it is evident, that the oblique muſcles will not be ſhortened one-fifth of their length till the point A has deſcended to D, or to number 3; or the oblique muſcles will, with the ſame degree of contraction, move the point A three times farther than can be done by ſtraight muſcles of the ſame length.

In the next place, we may eaſily demonſtrate, that the extent of the motion produced by the co-operation of oblique muſcles, increaſes with their greater degree of obliquity.

Thus, let us compare the extent of motion, produced by the pair of oblique muſcles AD and AE, (T. 2. fig. 5.) with that of the ſtill more oblique pair of muſcles repreſented in the ſame figure by the lines 1 D and 1 E. Let the muſcles

AD and AE be ſuppoſed to move the point A to number 1, and let the muſcles 1 D and 1 E be ſuppoſed to move number 1 to number 2, or through a like ſpace. It is evident, that in the triangles 1 I E and 2 II E, the angles 1 I E and 2 II E are equal; but as the angle 2 E II is larger than the angle 1 E I, the angle II 2 E muſt be leſs than the angle I 1 E. Hence, as the ſides of triangles are longer in proportion to the width of the oppoſite angles, the ſide I E will be longer in proportion to 1 E, than the ſide II E is in proportion to 2 E. The muſcular fibres, therefore, AD and AE, in bringing the point A down to number 1, will loſe more, in proportion of their length, than the more oblique fibres 1 D and 1 E will do in moving number 1 to number 2.

To prove this by calculation, let us ſuppoſe the muſcle to be ſtill repreſented by the hypotenuſe of a right angled triangle, five inches in length, and capable of ſhortening itſelf one inch, and that one of the other ſides meaſures four inches, and that the third ſide meaſures three inches. But let the ſide 3 form the baſis of the triangle, and the ſide 4 its perpendicular, as in T. 2. fig. 7.

In this caſe, the ſquare of the hypotenuſe, when it has ſhortened itſelf one inch, will be 16, from which deducting 9, the ſquare of the baſis, the number 7 remains for the ſquare of the perpendicular. But the ſquare root of that number being more than 2½, the oblique muſcles, ſhortened one-fifth, cannot bring the point A down 1½ inch, or to B, or cannot move the point A half ſo far as they were ſhewn to do, when the obliquity was greater, by making the baſis 4 inches and the altitude 3 inches.

On

Or let us, on the other hand, increaſe the obliquity, as in T. 2. fig. 8. by ſuppoſing two right-angled triangles, ſo conſtructed, as that their hypotenuſes meaſure 13 inches, their baſes 12, and altitude 5 inches, and that the hypotenuſes repreſent two oblique muſcles. It is plain, that when theſe have ſhortened themſelves one inch, or one thirteenth part of their length, they will move the point A through a ſpace of five inches, or five times farther than ſtraight muſcles of the ſame length, ſhortened in the ſame proportion, could do.

Hence, as the obliquity of an oblique muſcle is gradually increaſing during its action, its force is diminiſhing; while its effect, of producing extenſive motion, is increaſing. Thus, a muſcle, repreſenting the hypotenuſe of a right-angled triangle, whoſe ſides are to each other as the numbers 3, 4, and 5, and the altitude 3, by ſhortening itſelf half an inch, does not move its inſertion one full inch; but if it is ſhortened another half inch, its inſertion is moved through a ſpace of upwards of two inches more. When it begins to act, it has three-fifths of the ſtrength of a ſtraight muſcle of the ſame ſize; but when it acts again, after having moved its inſertion the ſpace of an inch, it has two-fifths only of the ſtrength of the ſtraight muſcle.

To illuſtrate what I have been demonſtrating, I uſed, after diſſecting and demonſtrating the recti muſcles of the abdomen, to cut their ends off from the oſſa pubis, and to apply them to the tops of the oſſa ilia, ſo as to repreſent oblique muſcles; and from this I was led to make the remark, that if both the two external oblique, or the two internal oblique muſcles, or all theſe, acted at once, the obliquity of the one balancing the obliquity of the other, the trunk of the body would be bended ſtraight forwards, and that flexion made by them might be greater than that made by the recti muſcles, which, at firſt ſight, ſeemed to be more ſuited to the purpoſe.

In like manner, I uſed to take out ſeveral of the ribs, with their intercoſtal muſcles; and after ſhewing the two layers of theſe muſcles laid obliquely, and decuſſating each other, I uſed to diſſect ſome portions of the two layers, in ſuch a manner as to repreſent oblique muſcles, with their origins at a diſtance from each other, but their inſertions meeting in a point, or with their inſertions, as well as their origins, at a diſtance from each other.

In the back part of the ſpine, I very particularly demonſtrated the obliquity of many of the muſcles, ſome of which are called ſemiſpinales, becauſe one end of them only is fixed to the ſpinal proceſſes, and the fibres paſſing obliquely, the other end of them is fixed to the tranſverſe proceſſes, or other parts of the neighbouring vertebræ.

In the extremities, I not only carefully demonſtrated the obliquity of the fleſhy fibres, in the half and whole penniform muſcles, but pointed out their connection with their tendinous aponeuroſes, the different direction of the tendinous and fleſhy fibres, and the uſes of the aponeuroſes and tendinous ſheaths; and that, by means of the ſheaths, there was ſo little difference between the length of the muſcles in the bended and extended ſtate of the member, that ſhort fleſhy fibres, eſpecially when placed obliquely, could produce a very extenſive motion.

But in treating of particular parts, I dwelt chiefly on the ſtructure and effects of the intercoſtal muſcles, as a variety of opinions concerning their operation has, in the courſe of the laſt hundred years, been propoſed, and as no author had explained the reaſon of the obliquity of their fibres, nor of their being diſpoſed in two layers of decuſſating fibres.

That their ſtructure might be fully underſtood, I firſt laid bare the ſurface of the external intercoſtal muſcles, and between the next two ribs, I cut off the external intercoſtals, to ſhew the internal, as in T. 3. fig. 4.

In another ſpace, I ſhewed a ſmall bundle of the external intercoſtal, decuſſating a ſimilar bundle of the internal intercoſtal, and forming a figure like the letter X, but in which the ſtroke repreſenting the external muſcle is more oblique than the other; for the internal intercoſtals are leſs oblique than the external. See T. 3. fig. 5.

Then I diſſected ſmall bundles of the external and internal intercoſtals, with their origins at a diſtance from each other, but their inſertions meeting in a point, in the rib above or in the rib below, ſo as to form triangles, of which the rib made the baſe, (ſee T. 3. fig. 6. and 7.); or I diſſected them with their inſertions, as well as their origins, at a diſtance from each other, as in T. 3. fig. 8.

In the laſt place, I demonſtrated a part of the ſtructure which has not been ſufficiently examined by authors; to wit, that the cartilages between the ribs and the ſternum, with the exception of the cartilage of the firſt rib, are not fixed to the ſternum in the ſame manner as to the ribs; for the rib, which is hollowed, receives the cartilage, and is ſo firmly united to it, that in a recent ſubject, they cannot be ſeparated without lacerating the cartilage; but the inner part of the cartilage is tied by a capſular ligament to the edges of the pit in the ſternum, and the concave part of the pit is connected by fine cellular threads only to the end of the cartilage, ſo that the cartilage and ſternum may, after cutting the capſular ligament, be ſeparated from each other without tearing the cartilaginous fibres. Hence, when the ribs are moved, the capſular ligament is twiſted, and the end of the cartilage rolls upon the ſternum. See T. 3. fig. 1, 2, 3. and 9.

AFTER fully explaining the ſtructure, I endeavoured to prove, as Dr HALLER had done, but with ſome additional arguments, that both rows of intercoſtal muſcles conſpired to elevate the ribs, or that they were muſcles of inſpiration; and that, when the intercoſtal muſcles alone acted, and the ribs were not forcibly kept down, they could have no other effect; and that all the ribs in inſpiration were moved upwards uniformly.

THE chief circumſtances which prove beyond a doubt, that the two rows of intercoſtal muſcles conſpire in elevating the ribs, are,

1. THAT the firſt rib is ſo much fixed at both its ends, as to be almoſt immoveable, and its cartilage, inſtead of being connected to the ſternum by a capſular ligament, or articulated with it in the ſame manner as the cartilages of the other ribs, grows as firmly to the ſternum as to the rib. See T. 3. fig. 9.

2. THAT the ſecond rib is more fixed than the third, and the third more fixed than the fourth, and ſo on downwards.

3. THAT as the ribs, from the firſt rib downwards, grow gradually longer, and deſcribe portions of larger circles, we may obſerve, that in general, or when we examine a middle portion of the intercoſtal muſcles, or a portion half-way between the ſternum and vertebræ, the inſertion of the lower end of the portion is at a greater diſtance from either end of the lower rib, or from a ſtraight line drawn between the two ends of that rib, than its origin in the rib above is from the two ends of that rib, or from a ſtraight line drawn between them. Hence, whether we conſider the head of the rib, connected with the vertebræ as its centre of motion, or whether we conſider the rib as moving upon a ſtraight line or axis drawn between its two ends, it follows, that a muſcle placed between two ribs acts with a longer lever upon the under rib than upon the upper one, and therefore muſt elevate the under rib. That the force of this argument might be more readily underſtood, I have laid leaden probes along each of the ſeven uppermoſt ribs of an adult ſubject, from the vertebræ to the ſternum, and have repreſented their lengths and curvatures in T. IV. The crooked continued lines repreſent the lengths and curvatures of the different ribs and their cartilages. The ſtraight dotted lines repreſent the diſtances between their heads and the ſternum. The continued perpendicular line repreſents the diſtance of the middle of each rib from a ſtraight line drawn between its two ends. The numbers 1, 2, 3, 4, 5, 6, 7, expreſs firſt, ſecond, &c. ribs, of which the firſt is the ſhorteſt and innermoſt, and the ſeventh the longeſt and outermoſt. The other numbers annexed denote eighths of an inch.

4. To determine the effect of the contraction of any muſcle, I apprehend we need only to obſerve in the dead body what the ſituation is in which the muſcle in queſtion is relaxed. Applying this rule, we ſhall find that the whole intercoſtal muſcles, internal as well as external, are ſhortened when we elevate the ribs, and place them in that ſituation in which we find they are in inſpiration.

5. If the internal intercoſtal muſcles had been intended for the depreſſion of the ribs, we certainly ſhould not have found them continued to the ſternum, becauſe their anterior ends are fixed above to the edge of the ſternum, or ſo near to the inſertion of the cartilage of the upper rib in the ſternum, and their inferior ends are, in conſequence of their obliquity, fixed to the under rib ſo much farther from the ſternum, that they muſt act upon the under rib with more advantage of lever, or are intended for its elevation.

On the other hand, if the internal intercoſtals had been intended for the depreſſion of the ribs, we certainly ſhould have found them continued backwards to the ſpine, becauſe, from their obliquity, their under end would have been fixed to the vertebræ or nearer to the head of the rib, and their upper end at ſuch a diſtance from it, that this portion of the muſcle would have been better calculated than any other portion of it for the depreſſion of the rib.

6. In a few experiments which I made on living animals, ſoon after I began to ſtudy anatomy, and which I repeated afterwards, particularly in 1770, I ſaw plainly, as Dr Haller had done, that both rows of intercoſtal muſcles were in action during inſpiration.

AFTER proving, that both rows of intercoſtal muſcles conſpire in elevating the ribs, I uſed to point out the fallacy of the demonſtrations, by which BAYLE, HAMBERGERUS, and others, have pretended to prove that the internal intercoſtal muſcles depreſs the ribs. The machine they deſcribe as repreſenting the ribs, vertebræ and ſternum, reſembles very exactly two wooden rulers, A, B, kept parallel by two pieces of braſs, C and D, ſuch as are uſed for drawing parallel lines; and the two layers of the intercoſtal muſcles are repreſented by the threads EF and HG, paſſing obliquely from the one ruler to the other, and decuſſating each other. See T. 2. fig. 9.

LET C, one of the pieces of braſs, repreſent the vertebræ, and the other piece D the ſternum. Let A repreſent the uppermoſt rib on the right ſide of the body, and B the ſecond rib. Let EF repreſent the external, and HG the internal intercoſtal muſcle.

THEN let C, repreſenting the immoveable vertebræ, be held faſt, and let EF be pulled or ſhortened, they tell us that the ſecond rib B muſt be more affected than the firſt, becauſe the lower end of the muſcle being at a greater diſtance from C than the upper end of it, the muſcle will act upon the ſecond rib with a longer lever, and therefore that the external intercoſtal muſcles muſt elevate the rib.

BUT when the internal intercoſtal muſcles, repreſented by HG, are ſhortened, they obſerve that matters will be reverſed; and as their origin in the firſt rib is farther from the vertebræ, or centre of motion, than their inſertion in the ſecond rib, that having a longer lever, they muſt ſerve to pull the firſt rib down.

Accordingly the rulers, on pulling alternately the threads EF and HG, will be moved alternately upwards and downwards.

But to ſhew the fallacy of this, I need only to add to what has been before obſerved, that we can perform a full inſpiration, without bringing the upper rib, or top of the ſternum, upwards or nearer to our head; whereas the pretended demonſtration reſts entirely on the ſuppoſition, that all the ribs, not excepting the firſt and ſternum, have a large play upwards and downwards alternately. Stop the play of the firſt rib, or ſuppoſe it to be fixed in its place, which is the fact, and the boaſted demonſtration is annihilated.

The late opinion of Sabatier *, that both rows of intercoſtal muſcles ſerve for expiration, and that the ribs are elevated by the ſcaleni and ſerrati poſtici ſuperiores, which are fixed to a few only of the upper ribs, ſcarcely merits a comment. It is refuted by what is above mentioned, and by the want of the external intercoſtals near to the ſternum, and of the internal near to the ſpine; for intercoſtals at thoſe places would have ſerved to depreſs the ribs more powerfully than in any other part of the thorax.

Let us now conſider the purpoſe,

First, Of the obliquity of the fibres in the intercoſtal muſcles, and,

Secondly, Of their being diſpoſed in two layers, the fibres of which decuſſate each other.

It

* See Anatom. T. 3. p. 465. 7. m.

It is evident, that the obliquity of the fibres here is not intended to increaſe their number, or the ſtrength of the muſcle, becauſe the fibres would have been more numerous if they had paſſed directly from the one rib to the other, or had been inſerted into the ribs at right angles.

I apprehend, therefore, that we are to explain the reaſons of the ſtructure in the following manner:

Nature, in order to give protection to the heart and lungs, has formed the ribs as broad and flat as poſſible, or left no more ſpace between them than is required for lodging muſcles for their motion in reſpiration. Conſiſtently with this view, as the ribs are fixed at both ends, ſo that they cannot be moved backwards and forwards, but are confined to motion upwards and downwards, remaining nearly parallel to each other, oblique muſcles are preferred to ſtraight; for if the former can, as I have before demonſtrated, perform more extenſive motion than the latter, even where both are of the ſame length, they muſt have a ſtill greater effect, where the two kinds of muſcles are confined between the ſame parallels.

Thus, ſuppoſe the direct diſtance, or perpendicular drawn from one rib to another, to be repreſented by three parts, and that the intercoſtal muſcle, in conſequence of its obliquity, meaſures five ſuch parts, and that each of theſe is capable, when in action, of ſhortening itſelf one-fifth part of its length; it appears from the demonſtration, that the oblique muſcle can move the rib through a ſpace five times greater than the ſtraight muſcle can do.

On accurate menſuration, I found the length of the intercoſtal muſcle to be one inch and a half, the perpendicular line one inch, and the baſe about one and one-eighth inch. Hence, calculating on the ſuppoſition, that the muſcular fibre, in action, ſhortens itſelf one-fifth of its length, it will be found, that the intercoſtal muſcles, in conſequence of their obliquity, produce a greater motion of the ribs than perpendicular muſcles could have done, nearly in the proportion of 35 to 12.

The only point remaining to be explained, is, why nature hath formed two layers of intercoſtal muſcles decuſſating each other.

The purpoſe of this, ~~I apprehend,~~ beyond doubt is to render the motion of the rib upwards as direct as poſſible, and to prevent it from being drawn or preſſed forwards upon the ſternum, or backwards upon the vertebræ, ſo much as, by its friction, to interrupt the freedom of its motion.

Upon the whole, by the obliquity of the intercoſtal muſcles, the motion of the ribs is very much greater than could have been performed by ſtraight muſcles placed between them: At the ſame time, by their conſiſting of two layers, or two muſcles decuſſating and balancing each other, the motion of the ribs, upwards and downwards, is as direct, and with as little friction, as if it had been performed by ſtraight or perpendicular muſcles.

APPENDIX,

In which the Pretenſion of Dr GILBERT BLANE, that he *firſt* demonſtrated the ſame Effect to be produced by Oblique Muſcles as by Straight Ones with a leſs proportional Decurtation of Fibres, is proved to be unfounded.

AS Dr GILBERT BLANE, phyſician in London, who attended my Courſe of Lectures on Anatomy and Surgery two years, *to wit*, 1769 and 1770, has, in the Croonian Lecture on Muſcular Motion, read by him at the Royal Society, November 13. and 20. 1788, and printed in London in 1790, given a geometrical demonſtration, that the ſame effect is produced by oblique muſcles, with a leſs proportional decurtation of fibres, than if the ſame motion had been performed by a direct power, without acknowledging that he had learned from me any thing upon the ſubject, I found myſelf under the diſagreeable neceſſity, after I had treated of it in my uſual manner, in my laſt Courſe of Lectures, to mention Dr BLANE's publication, and then to read Notes I had written on the ſubject, ſo far back as winter 1759-60, and other notes which happened to be taken from my Lectures, by Mr THOMAS THORBURN in 1770, during the laſt year Dr BLANE attended my Courſe; and of which many copies have, ſince that time, been written and circulated among the ſtudents.

DR BLANE, who was informed of this, as I ſuppoſed and wiſhed he ſhould be, becauſe his brother was, laſt winter, one of my pupils, has, ſince that time, *to wit*, on July 27. 1792, written a letter to Mr BENJAMIN BELL, Surgeon in Edinburgh, on that ſubject, which Mr BELL thought himſelf authoriſed to ſhew me; and I received it from him on the 21ſt of Auguſt 1792.

THAT what Dr BLANE has publiſhed or written on the ſubject may be fully underſtood by the Reader, I ſhall now ſubjoin the part in queſtion of Dr BLANE's Lecture and Letter, then add the notes I referred to, taken from my Lectures in 1770 by Mr THORBURN, with the atteſtations of gentlemen diſtinguiſhed by their genius and learning, who have done me the honour of attending my Courſes of Lectures, before, when and ſince Dr BLANE did ſo; and I ſhall conclude with a few Remarks upon the whole.

An EXTRACT from THE CROONIAN LECTURE ON MUSCULAR MOTION, read by Dr GILBERT BLANE at the Royal Society, November 13. and 20. 1788, corrected, enlarged and printed at London, 1790, in 4to, pages 55, 56, 57.

" BUT the advantage, or rather compenſation of obliquity, which I mean particularly here to demonſtrate, is, that the ſame effect is produced with a leſs proportional decurtation of fibres, than if the ſame motion had been performed by a direct power. BORELLI has eſtimated geometrically the loſs of power from oblique action, but ſeems to have overlooked this compenſation of it, which is not inconſiderable, when we reflect that there is thereby a ſaving of contraction, and conſequently of fatigue. This can be rendered an object of geometrical proof, and I here ſubjoin a demonſtration of it, which I made out, when engaged in the ſtudy of anatomy, eighteen years ago.

" LET the line AB, in the annexed diagram (T. 2. fig. 10.) repreſent a moveable bone, and the line CD a fixed bone parallel to it. Let FE, perpendicular to theſe lines, repreſent a muſcle acting in its own direction, and the lines GE, HE repreſent two muſcles acting obliquely, and producing, by a diagonal action, the ſame effect as the other. If the

" bone AB be brought to the ſituation ab, by the action of the
" muſcle FE, the muſcle will then be in the ſituation FK. If
" the bone is brought into the ſame ſituation by the action of
" the muſcles GE, HE, theſe muſcles will then be in the ſi-
" tuation GK, HK.

" THE propoſition to be demonſtrated is, that the line GK
" bears a greater proportion to the line GE, than the line FK
" does to the line FE; for FK is to FE as GL is to GE,
" (Euc. Elem. B. vi. prop. 2.) and the angle ELK, being leſs
" than a right angle, the angle GLK, which is adjacent to it,
" muſt be greater than a right angle; and the angle GKL be-
" ing in the ſame triangle GLK, muſt be leſs than a right
" angle. The line GK, therefore, which ſubtends the greater
" angle, is greater than the line GL, ſubtending the leſſer,
" and therefore bears a greater proportion to GE. But the
" line GL is to GE, as FK is to FE; and therefore GK
" bears a greater proportion to GE, than FK does to FE;
" that is, the fibres of the muſcles acting obliquely, ſuffer a
" leſs proportional decurtation than thoſe of the muſcle act-
" ing directly.

" IT is farther obvious, that the more oblique the action
" becomes, the greater ſaving there will be of contraction;
" for in moving the line ab towards CD, the line FK dimi-
" niſhes in a ſwifter ratio than the line GK, and when the
" former has vaniſhed, the latter is in the ſituation GF."

COPY of a LETTER from Dr GILBERT BLANE, Phyſician in London, to Mr BENJAMIN BELL, Surgeon in Edinburgh. Dated London, July 27. 1792.

DEAR SIR,

I WAS a good deal hurt at learning, ſome time ago, that Dr MONRO had made very free with my name in his laſt Courſe of Lectures, reſpecting ſome Remarks of mine in a Lecture on Muſcular motion, before the Royal Society, of which I printed a few copies for my friends. If the Doctor's animadverſions were to reach only thoſe to whom I am known, I ſhould not pay much attention to them, as they know me to be incapable of ſo unworthy a conduct; but as the greater part of his auditors are moſt probably ſtrangers to me and my character, I cannot entirely overlook this imputation.

THE queſtion is about a matter of fact, which paſſed one or two and twenty years ago, relative to ſome remarks on the obliquity of Muſcles, which the Doctor alleges I have borrowed from him, without acknowledging it. As I originally owed all my anatomical knowledge to Dr MONRO, there can be no doubt that I was led to conſider this ſubject from that part of the Courſe which related to the muſcles, particularly the intercoſtals; and, as a Profeſſor in a General Courſe communicates all that is known on the ſubject, whether diſcovered by himſelf or others, I do not know whether Dr MONRO delivered his remarks on this ſubject, as his own or not; nor would this make much to the preſent argument, as it would be equally wrong in me to arrogate the obſervations of any other.

WHAT I mean to allege in anſwer, is, 1. That Dr MONRO, during my attendance, never made uſe of any geometrical demonſtration, but only ſuch groſs illuſtrations as were adapted to his mixt audience. *2dly*, Dr MONRO's obſervations were entirely confined to the intercoſtal muſcles, the mechaniſm of which I have made uſe of, rather as an exception, than an illuſtration of my doctrine, the eſſence of which conſiſts in eſtabliſhing the univerſality of oblique motion in muſcles, and the advantage of this poſition in multiplying their number, and ſaving fatigue, by leſſening their proportional decurtation. So far as I know, theſe remarks are wholly my own. If it ſhould be alleged that they are only an extenſion or improvement of what was delivered in the Lectures, I am ready to allow this; for, as I have ſaid before, it was from them I derived all my original anatomical knowledge; but if this mode of arguing is adopted, there is an end to all merit and all future reſearch. For what ſubject is there to which one can turn their thoughts, on which a good deal has not been already ſaid? With regard to the geometrical demonſtration, I can adduce a fact which I think cannot fail fully to convince Dr MONRO that it never could be my purpoſe to defraud him of any right he had to the remarks in queſtion. In ſummer 1774, there was a ſmall club of ſtudents who held weekly meetings at each others rooms, to converſe upon medical ſubjects in Latin, as a preparatory exerciſe to graduation. I was a member of this club, and one evening I brought with me the mathematical propoſition and remarks, which I ſhewed as my own, and they were canvaſſed accordingly. I farther remember, that the meeting at which this paſſed, was held at the apartment of Mr Turner, a gentleman from Yorkſhire, who lodged in the corner of the Lower College, at the houſe of Mr Innes, who ſo long and ably aſſiſted Dr MONRO in his diſſections. I appeal to any one whether, in an aſſembly of intelligent ſtudents, who had themſelves recently attended the Lectures, I could have had the effrontery to paſs for my own, what had been a part of the preceding Courſe of Lectures, of which they were as much maſters as myſelf.

As it would give me great pain to have any altercation with Dr MONRO, to whom I wiſh to retain that reſpect which I owe to my old maſters, I ſhould be much obliged to you, when you happen to meet the Doctor in conſultation or otherwiſe, if you will bring on this ſubject; which I think, from what I have ſaid, you can ſtate in ſuch a manner as to ſatisfy him.

WHEN I firſt began, in 1759, to deliver, without the aſſiſtance of my father, the whole Courſe of Lectures on Anatomy and Surgery, I had had little time for preparation, as I had been called home on account of my father's indiſpoſition, much ſooner than I expected. My notes were therefore very ſhort, on almoſt every part of my Courſe. And on this ſubject I did not, till of late years, collect in one Lecture all the circumſtances I have mentioned in the foregoing paper. It appeared to me improper to deliver ſuch a Lecture, before the ſtudents had ſeen the ſtructure of the muſcles, and equally improper to delay taking any notice of the effects of the oblique muſcles, till I had finiſhed the myology. I therefore uſed, generally in my third Lecture on myology, after demonſtrating the abdominal muſcles, to mention the more extenſive motion produced by oblique, than by ſtraight muſcles. I obſerved that the oblique muſcles of the abdomen concurring, might bend the body farther forwards, or draw farther downwards the cartilage enſiformis, than the recti muſcles, which were ſuppoſed to be, and, at firſt ſight, appeared to be better fitted for that purpoſe; and that, if the recti muſcles had paſſed from the enſiform cartilage, obliquely outwards, to the tops of the oſſa ilia, they would, with the ſame degree of ſhortening, have produced a more extenſive motion. And ſometimes, in order to be better underſtood, I cut them off from the oſſa pubis, and turned their lower ends ſideways to the tops of the oſſa ilia. Sometimes I added the geometrical and arithmetical demonſtration, by drawing with chalk on a black board, fig. 4. and afterwards fig. 6. or converting fig. 4. into fig. 6. by lengthening the perpendiculars. But, generally, I referred theſe demonſtrations till I had ſhown the intercoſtals; where not only the obliquity, but decuſſation of the muſcular fibres, fell to be explained.

In treating various other parts of the myology, and particularly of the muſcles on the poſterior part of the trunk, I never failed to point out the various effects of the obliquity of the muſcles.

When I came to the muſcles of the hands and feet, where the obliquity of the fibres, in many of them, is ſo remarkable, that they have been called penniform, I thought it ſufficient to ſhow the ſtructure accurately, and to put them in mind of, without repeating my demonſtration.

I shall, now, copy verbatim, firſt, my own notes, on which I lectured in 1759-60; and then thoſe written at different times by other perſons.

NOTES,

N O T E S, &c.

[*The following is a Literal Copy of Notes written by Dr* MONRO *in Winter* 1759-60.]

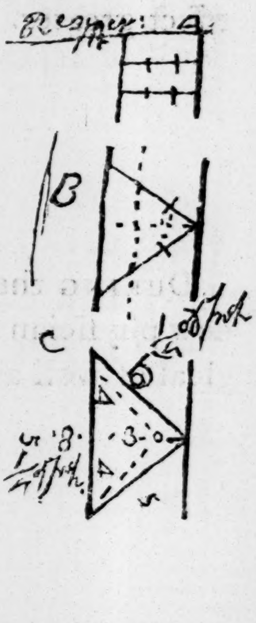
1760. LECT. 17. ON LARGE BODY.

Of RESPIRATION.

AFTER deſcribing, in ſix quarto pages, the motions of the Thorax, and endeavouring to prove that both rows of Intercoſtal muſcles elevate the ribs, I wrote as follows:

"The intention of nature in this is very ſimple and evident. By oblique fibres, a much greater effect is produced than by ſtraight fibres; or two bones may be brought much nearer to each other. By ſtraight fibres you can only approach two bones one third nearer to each other; whereas, by oblique fibres they may be brought cloſe to each other. This is evident by comparing A with C." *

1760.



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* *N. B.* Theſe three figures are copied exactly by the engraver, from the original manuſcript, written in 1760, on the margin of which they are drawn.

1760. Of the Courſe, v. 8. p. 86, 87.

LECT. 68. of the Courſe.

REMARKS on the Muſcles that move the Head and true Vertebræ.

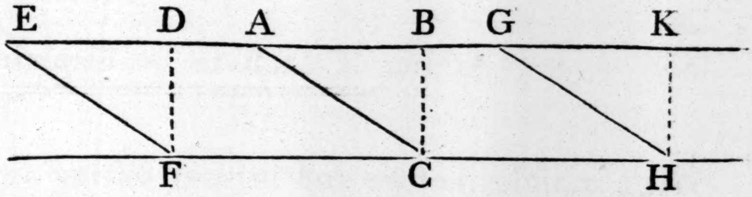
Action of each to be Explained.

ALL muſcles before and behind, acting at once, pull directly forwards or backwards.

PART of theſe before and behind, acting at the ſame time, pull the head to a ſide, as ſterno-cleido-maſtoid and ſplenius with trachelo maſtoid; and, as the chief muſcles are oblique, the flexions are greater than by ſtraight fibres. Neck moved by above mentioned muſcles, with addition of longus colli, tranſverſalis colli, intertranſverſalis, interſpinales, ſpinales, part of muſculus angularis, ſcaleni, which operate in a ſimilar way, the oblique fibres of ſeverals making the extent of their effect greater.

DURING the years 1762, 1763, 1764, DR JOHN HAYGARTH, now phyſician in Cheſter, attended my lectures, and as phyſiological as well as practical matters, attracted his notice; I received from him, if I recollect rightly, two letters on the ſubject in queſtion. And, fortunately, I have found part of the firſt letter, which I was afraid I had deſtroyed many years ago with other papers I thought it of no uſe to preſerve. In this fragment, he writes in the following words:

To Dr Monro, *jun.*



Your account of the advantage derived from the united action of both the intercoſtal muſcles croſſing one another, in procuring a greater extent of motion, at firſt pleaſed me exceſſively, the ſolution is ſo extremely ſimple and beautiful. On further reflection, it appeared to me that this greater extent of motion might be procured from fibres running in one direction, and was owing to the obliquity, not the croſſing of the fibres. Let EK and FH repreſent two bones, AC a muſcular fibre connecting them together in an oblique direction. Draw the perpendicular BC. Let AB be two inches long, AC three inches. If the fibre AC contracts one-third of its length, will it not coincide with AB, and bring the points B and C together? Now, if we imagine other oblique fibres EF, GH, will not they——

From this fragment it appears that I had demonſtrated the great effect of oblique fibres in the intercoſtal muſcles; but, as I ſpeak in lecturing without notes before me, I either had neglected to explain the reaſon of there being two layers of intercoſtal muſcles decuſſating each other; or the explanation I had given had not been attended to by Dr Haygarth.

I ANSWERED this letter, by endeavouring to explain the reaſon of two ſuch layers, in the way I have done above, and received a ſecond letter from the DOCTOR, expreſſing his ſatisfaction with the ſolution I had given him.

VERY lately I have written to Dr HAYGARTH, and received his anſwer in the following words :

LETTER from Dr A. MONRO to Dr JOHN HAYGARTH.

DEAR SIR,

As I intend to publiſh very ſoon, ſome Obſervations on the Muſcles, and wiſh to aſcertain the time at which I firſt demonſtrated, that a greater extent of motion can be produced by oblique muſcles than by ſtraight ones, I give you this trouble, as I recollect that you paid particular attention to the ſubject, whilſt ſtudying in this Univerſity. I have in my manuſcript introduced the mention of your name in the following terms :

During the years 1762, *&c.*

THE favour I beg is, that you will collect from your notes, or recollect from memory what then paſſed on this ſubject, and write it to me as ſoon as you conveniently can, which will much oblige,

Your moſt obedient ſervant,

ALEX. MONRO.

Edinburgh, October 27. 1792.

To Dr John Haygarth.

LETTER from Dr JOHN HAYGARTH to Dr ALEX. MONRO.

DEAR SIR, *Cheſter, October* 31. 1792.

I HAVE fortunately preſerved a copy of the letter to which you refer. I attended, and with very high ſatisfaction, three Courſes of your Anatomical Lectures. The letter was dated the 2d of February 1764, during my attendance on the ſecond Courſe. The fragment you have preſerved very exactly correſponds with my copy. " Your account, &c. brought into " contact.' The remainder of my letter ſtates ſome difficulties concerning the alterations by which the eye adapts itſelf to view objects at great and little diſtances, and concerning ſcrophulous tumours, as they affect the lymphatic glands.

I WELL remember, that in a very obliging manner, you took the trouble to explain the difficulties I had ſuggeſted. A ſecond letter to expreſs my thanks and conviction might very probably be written; but at this diſtance of time I do not recollect it.

IF my teſtimony can be of any ſervice to aſcertain the time when you demonſtrated that a greater extent of motion can be produced by oblique muſcles than by ſtraight ones, you are every way well entitled to it. I ſhall give it with peculiar pleaſure.

LETTER from Dr MONRO to Dr HAYGARTH.

DEAR SIR,

I RECEIVED the favour of your anſwer to my firſt letter, from which I obſerve, that I had not been ſufficiently explicit in acquainting you that I had loſt that part of your letter written in 1764, which followed the extract I ſent you.

I AM therefore obliged to trouble you again, to beg,

1. THAT you will ſend me what followed, to complete the fragment, or that you will ſend me the ſequel of your letter to the words "brought into contact."

2. To aſk whether you recollect the particular demonſtrations by mathematical figures and arithmetical calculations, which I then employed in proof of the more extenſive motions produced by oblique than by ſtraight muſcles.

3. WHETHER you recollect that I applied the demonſtrations not only to the intercoſtal muſcles, but to muſcles in general, and particularly to the abdominal muſcles, the muſcles on the back part of the head, and to the penniform muſcles of the extremities.

As I have engaged to read a paper on this ſubject to the Royal Society of this place, you will add much to the obligation, by ſending me your anſwer as ſoon as you can conveniently.

I am,

Dear Sir,

Your very humble ſervant,

A. MONRO.

Edinburgh, *January* 5. 1793.

LETTER from Dr HAYGARTH to Dr MONRO.

DEAR SIR,

I AM indeed much concerned, that the inaccurate hurry with which my anſwer to your letter was written, has given you the trouble of writing another. I did not then obſerve, that the fragment of my former letter in your poſſeſſion wanted a few words at the concluſion, to render the ſenſe complete.

AFTER "E, F, G, H," add, "will they not bring the "point I to touch F, and the point K to touch H; and will "not the two bones be brought into contact?"

ON the farther ſubject of your enquiry, I have ſearched with attention the notes taken at your Anatomical Lectures, which I have ſtlll preſerved, but without ſucceſs. They are indeed ſo very imperfect, eſpecially on the anatomy of the muſcles, as to afford no deciſive evidence on either ſide of the queſtions you aſk.

NOR, 2*dly*, At this diſtance of time can I recollect any demonſtrations, or mathematical figures and arithmetical calculations to prove the more extenſive motion produced by oblique, than by ſtraight muſcles.

3*dly*, NOR do I remember that you applied ſuch demonſtrations to the intercoſtal, abdominal, occipital, or dorſal muſcles, or to the penniform muſcles of the extremities.

BUT I wiſh particularly to declare, that neither the imperfection of my memory, nor the imperfection of my manuſcript, can afford any proof, or any very ſtrong preſumption that you did not teach theſe doctrines in 1762–1765, when I had the pleaſure to attend your Lectures; a pleaſure which is always recollected with the greateſt ſatisfaction and gratitude, by

Your obliged friend and ſervant,

JOHN HAYGARTH.

Cheſter, January 12. 1793.

IN the years 1769 and 1770 Dr GILBERT BLANE attended my Lectures; and in the laſt of theſe two years Mr THOMAS THORBURN did ſo: He likewiſe attended my Lectures in 1773. About the ſame period, Mr THORBURN attended ſeveral of the other Medical Profeſſors, and wrote down their as well as my Lectures, which, for a number of years afterwards, were copied and ſold, or lent out to be copied by the ſtudents. And, on the loweſt computation, that twenty copies of my Lectures have ſince that time been written annually by the ſtudents, there muſt be now extant more than four hundred copies of them; for they have been handed down to this time, with fewer corrections and additions than might have been expected.

ON the 8th of November 1774, I purchaſed a copy of Mr THORBURN's manuſcript, written in ten volumes, by Mr JOHN WILSON, who, from being lame, had the conceit of calling himſelf *Claudero*, and was beſt known by this name.

He ſubſcribes the laſt page of the laſt volume in the following words:

"FINIS.

"Edinburgh, April 29. 1774.

"CLAUDERO *ſcripſit*."

I FOUND that Mr THORBURN, who had no knowledge of Anatomy when he began to write my Lectures, had not attempted to write my demonſtrations or deſcriptions of the parts; neither had he attempted to copy any of thoſe figures which, in this and in many other parts of my Courſe, I have been in the cuſtom of drawing with chalk upon a black board, in order to render my Lectures more intelligible to the ſtudents. In conſequence of this, his notes are particularly imperfect wherever the Lecture is connected with figures. Keeping this in view, let us next read what he had written.

MUSCLES *of the* ABDOMEN.

VOL. II. p. 348. "With regard to the oblique, their ac-
" tion may be reduced to the greateſt ſimplicity; for the ex-
" ternal oblique of the one ſide has its fibres in the ſame di-
" rection

“ rection with the internal of the other; ſo may be conſidered “ as forming one digaſtric muſcle; and when I draw down “ the thorax, or raiſe the pelvis with a degree of obliquity, “ theſe muſcles are employed; but in other reſpects their action is ſimilar to that of the recti; for if I act with them “ all, the ſlanting direction of the one balancing that of the “ other, the body is moved ſtraight.”

Vol. II. p. 350. “ With regard to the oblique, we find “ that the degrees of motion they perform are greater than “ could have been made by ſtraight muſcles. This is a “ circumſtance that is entirely overlooked; yet the influence “ of it is very powerful. The obliquity of the one muſcle balances the obliquity of the other, as I have ſaid, when the “ body is brought forwards, and here the action is a good deal “ more extenſive, than it otherwiſe would have been. Thus “ ſuppoſe an action to be required by the rectus, the rectus we ſhall ſay can ſhorten itſelf one-third of its length, “ and the thorax would have been drawn ſo much down; but “ when two ſuch muſcles are placed ſlanting, and the obliquity of the one balances that of the other, the action is “ much more extenſive. In this part of the body it is not “ often that we have occaſion for ſo great an extent of motion, “ but between the ribs for inſtance, where the ſpace left “ for motion is very ſmall, this contrivance is very uſeful “ and neceſſary.”

Vol. II. p. 437. “ The only difficulty here, and which “ has puzzled many anatomiſts, is to account for the obliquity of the intercoſtal muſcles: And next, why there are “ two rows of them? I apprehend this difficulty entirely “ depends upon anatomiſts not having obſerved a circum-

" ſtance of the utmoſt moment, and which we noticed be-
" fore, that wherever extenſive motion is required, oblique
" fibres are employed, the oblique fibres giving a greater
" play than the ſtraight, bring the two ribs much nearer, and
" in conſequence of there being two rows, the obliquity of
" the one balances the obliquity of the other, and raiſes the
" rib in an equal way, neither puſhing it backwards upon
" the ſpine, nor drawing it forwards upon the ſternum."

Vol. II. p. 466. " When we obſerve in what way the head
" can be moved, the combined action of the muſcles in mo-
" ving it is very ſimple. In general, every muſcle draws its
" two points as much as the articulation will allow in a
" ſtraight line; and all the muſcles having ſlanting fibres
" twiſt the head. But if a pair of theſe is in action at the
" ſame time, the obliquity of the one balancing the obliquity
" of the other, they ſerve the purpoſe of a ſtraight muſcle.
" So if I want to draw the occipital bone backwards in a
" ſtraight line, inſtead of employing muſcles which run very
" nearly in that direction, we can employ the muſculi tra-
" pezii, which run in a ſlanting direction, and the head moves
" in a diagonal line between them, and by their means we can
" draw the head much farther back than we could do by a
" muſcle ſent directly from the bottom of the neck to the
" occiput.

" If I mean to draw the head ſtraight forwards, I employ
" all the muſcles on both ſides, whether ſtraight or oblique,
" and in like manner, when I mean to draw it backwards.
" But for pulling it ſideways, we are obliged to employ an
" anterior and poſterior muſcle at the ſame time. So in or-
" der to bring the maſtoid proceſs down to the ſhoulder, I

" act with the ſterno-maſtoideus and the ſplenius muſcle, " which counteract one another as to obliquity, and, by a ſucceſſion of theſe motions, I can make my head deſcribe a " circle."

Vol. II. p. 470. " If we mean to raiſe the ſcapula, we employ two muſcles which decuſſate each other, the obliquity " of the one balancing the obliquity of the other, *to wit*, the " upper part of the trapezius and the levator ſcapulæ. If I " mean to pull it down, to preſs any thing under my arm, I " employ the lower part of the trapezius; for different portions of a muſcle may be employed at different times; and " with it, at the fore part, the ſerratus anticus major, particularly the portion of it near to the lower angle, which " move it farther than a ſtraight muſcle would have done. " If I mean to bring it backwards, I employ the whole of the " trapezius, but more eſpecially the lower and upper ends," *&c.*

Mr Thomas Thorburn, after finiſhing his ſtudies in this Univerſity, ſettled as a Practitioner of Phyſic in March, Cambridgeſhire, where he died twelve years ago. His ſon, Mr John Thorburn, who is ſtudying Phyſic, and has attended my Lectures this and the laſt winter, is in poſſeſſion of his father's original manuſcript, written in ſhort hand in 1770, which he has extended as accurately as he could, and favoured me with the following exact tranſcript from it.

Lect.

Lect. 27.—Muſcles of the Abdomen.

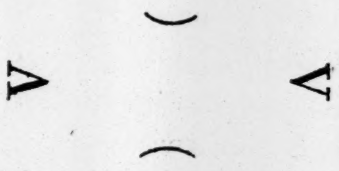
In a ſimilar way we explain the action of the oblique muſcles, making allowance for the obliquity of their fibres. The external oblique draws down the ribs in a ſlanting direction, pulls the body, and at the ſame time turns it over to a ſide, or twiſts the body. The internal oblique has a ſimilar effect; nay the external oblique of one ſide concurs with the internal oblique of the other in the ſame action, ſo we may conſider them as one digaſtric muſcle, or, they may be conſidered as a continuation of each other. So the operation is perfectly ſimilar, only, that the right external acts at the ſame time with the left internal and *vice verſa*. Or we may throw all the oblique muſcles into action, or a pair, and, the obliquity of the one being balanced by that of the other, we bring the body ſtraight forwards, as by the recti muſcles. I would only add, that by the oblique muſcles we can perform a greater flexion and motion, than by the recti. The rectus appears better calculated for bending the body forwards, but a much larger motion can be made by the oblique. Take any proportion of contraction, ſuppoſe one-third, the rectus muſcle acting, the cartilago enſiformis cannot deſcend lower than this. But, when the oblique muſcles act, the one balancing the other, can bring down the cartilago enſiformis nearer to the bones of the pelvis. So they really make a larger motion than ſtraight fibres can do. But in conſidering the thorax, where the effects of the oblique muſcles are more neceſſary than here, I ſhall conſider this.

LECT. 37.

IT only remains, that we determine the power by which the ribs are moved, and here authors have perplexed themſelves very much from not attending to the particular uſes of oblique fibres in the intercoſtal ſpaces, and from not rightly underſtanding the uſe of two rows of intercoſtal muſcles, and the two rows are given becauſe of the obliquity.

I APPREHEND that every difficulty may be eaſily removed, and one general point of much conſequence in our body is explained by it, *viz.* the great effect which oblique muſcles have in performing a more extenſive motion, than is poſſible by means of ſtraight fibres.

Now, I don't recollect that this has been explained by any author. I ſhall draw the abdomen, here is the cartilago enſiformis, and here are the oſſa pubis,



NEXT theſe two repreſent the oſſa ilia, and ſuppoſe a large flexion of the body is intended, and that I need to draw down the enſiform cartilage very near to the oſſa pubis. That cannot be done by the muſculi recti, which I next draw. Let us aſſign any particular length to the recti, that they are 10 inches long, and determine to what degree the common muſcles ſhorten in their action. Suppoſe one-fifth, or when the recti muſcles act, the cartilago enſiformis is brought down two inches. Now, I next draw two oblique muſcles of the exact length that I have ſuppoſed the recti, 10 inches long, and I conceive to myſelf, that theſe contract in the ſame proportion, when that is made, where will the enſiform cartilage be brought? It will be brought a great deal lower. When a workman wants to draw a right angle, he makes a triangle, one ſide of the triangle is eight, and call the other ſide ſix, if this angle be right, the long ſide is exactly 10. Now, if I ſhorten each of theſe fibres one-fifth, the obliquity of the one balancing the obliquity of the other, the cartilago enſiformis moves in the diagonal line, and can be brought down oppoſite the inſertion of the muſcles, or inſtead of moving two inches, it may be made to move ſix, which is three times more than it did formerly. Try the experiment with cords and weights, and you will be convinced that the oblique perform much greater motion. Now, with regard to the intercoſtal muſcles, their large motions depend upon this entirely, yet, the reaſon has not been given. The ſpace is ſo ſmall that a ſtraight fibre could not have given ſufficient play, hence, there is an obliquity. If there had been one row of oblique fibres only, the ribs would have been drawn either too much forwards or backwards, and the motion would not have been equal. But by throwing in two rows, the greateſt motion is performed, and the motion is perfectly equable, the ribs neither moving upon the vertebræ behind, nor the ſternum before, but moving parallel to each other. And as the upper ribs are more fixed than the under, when the intercoſtal muſcles are in action, there is a general motion upwards. For, ſuppoſing that by any power, the intercoſtal muſcles are thrown into action, the under rib moves towards the upper, and the uppermoſt of all is fixed by its ſtructure.

Lect. 40. Muſcles of the Head.

We have ſeen a great number of muſcles which are chiefly intended for the motion of the head, but few of them are upon the fore part, becauſe the muſcles of the jaws can at pleaſure act upon the whole head, and that with great advantage, in conſequence of the diſtance of the jaws from the condyles of the occiput the centre of motion. When we act only with the muſcles proper to the head, the different degrees of obliquity of the ſterno maſtoidei balancing each other, the head is drawn forwards, and ſo with reſpect to the muſcles behind, and in general, you will obſerve, that the obliquity extends the motion.

I act with the muſcles of one ſide, whilſt I bend the head forwards. I turn the face towards that ſide, and alternate the motion. I give a kind of rotation to the head. In like manner on the back part, I either with the action of all the poſterior muſcles, draw the head ſtrongly backwards, or acting with the oblique, I give it a rotation; or to take all the motions which we are capable of performing, I bend the head ſtraight forwards by the anterior ſtraight muſcles, and backwards by the poſterior, but at the ſide I employ an anterior and poſterior muſcle at the ſame time. So if I mean to preſs the ear down upon the ſhoulders, I employ the ſterno maſtoidius, and the ſplenius ſuppoſe of the ſame ſide, the obliquity of the one, balancing the obliquity of the other, the head is moved in a diagonal between them, and by a ſucceſſion of theſe, I can make my head deſcribe a circle.

Next let us attend to the motions of the ſcapula. Authors before Winslow, uſed to ſpeak of the motions of the ſcapula, upwards, downwards, backwards and forwards. Winslow has added to theſe a rotation of the ſcapula, and fond of this obſervation, he carries things too far, rejecting the direct motions. Nay, farther, he errs with reſpect to the manner of its rotation, conceiving it to roll upon an imaginary axis put through the middle. But from its connection to the clavicle, it is impoſſible it ſhould do this. We cauſe the ſcapula to roll upon the end of the clavicle, which is its centre of motion. Next we roll the clavicle itſelf upon the top of the ſternum, but that I can perform the motions upwards, downwards, backwards and forwards, is ſo evident, that we all experience it, and can point out the muſcles fitted to perform it. Firſt, the trapezius muſcle, and joined to this the levator ſcapulæ are fitted for making the motions of the ſcapula upwards, the obliquity of the one balancing that of the other, and preventing the rotation. Next, the whole trapezius muſcle being at once thrown into action, draws the baſis of the ſcapula towards the ſpine of the body. If again, I mean to make a motion of the ſcapula downwards, here are two muſcles in our view perfectly fitted for our purpoſe, the lower ſhare of the trapezius and the ſerratus anticus major; and to pull the ſcapula directly forwards, I ſlacken the trapezius, and act with the ſerratus anticus major, and balance it with the ſerratus minor or leſſer pectoral, to pull forward the coracoid proceſs; and by a ſucceſſion of theſe, I can move the ſcapula in a circle.

TRANSCRIBED from the original manuſcript lectures, taken in ſhort hand by Mr THOMAS THORBURN in 1770, by me, his ſon.

JOHN THORBURN.

Edinburgh, December 27. 1792.

As I underſtood that Mr BENJAMIN BELL and Mr JAMES RUSSELL, Surgeons in Edinburgh, had purchaſed copies of notes taken from my Lectures, without knowing by whom they were written, I aſked the favour of them to furniſh me with extracts from their manuſcripts on the ſubjects in queſtion. Mr RUSSELL purchaſed his copy in the year 1775 or 1776, and Mr BELL has had his copy about ſix years in his poſſeſſion. I found they were both written in the ſame, following words; but, not being copied by the ſame hands, the pages of the manuſcript are differently numbered.

PAGE $\frac{289}{224}$. I would only add, that by the oblique muſcles we can perform a greater flexion and motion than by the recti. Take any proportion of the contraction you will, ſuppoſe a third, when the rectus muſcle acts, the cartilago enſiformis cannot deſcend towards the os pubis more than one third; but when the oblique muſcles act, the one balancing the other, we can bring the cartilago enſiformis nearer to the bones of the pelvis in a greater proportion.

PAGE $\frac{345}{268}$. It only remains, that we determine the power by which the ribs are moved. And here Authors have perplexed themſelves very much, from not attending to the particular uſe of oblique fibres in the intercoſtal ſpaces, and from not rightly underſtanding the uſe of two rows of intercoſtal muſcles; and the two rows are given becauſe of the obliquity. I apprehend that every difficulty can be removed with the greateſt eaſe, and one general point of much conſequence in our body is explained by it, *viz.* the great effect which oblique muſcles have in performing a more extenſive motion than is poſſible by means of ſtraight fibres. Thus, ſuppoſe a large flexion of the body is intended, and that I want to draw down the enſiform cartilage very near to the oſſa pubis, that can very well be done by the muſcle recti. Suppoſe that theſe are ten inches long, and that they ſhorten one fifth, ſo that when the recti act, the cartilago enſiformis is brought down two inches; but ſuppoſe we uſe two oblique muſcles of the ſame length, and that they contract in the ſame manner, the enſiform cartilage will now be brought a great deal lower, the obliquity of the one balancing the obliquity of the other, the cartilage moves in the diagonal line between them; and inſtead of moving two inches only, it will be made to move ſix, that is three times more than it did formerly.

Now, with reſpect to the intercoſtal muſcles, their large motion depends upon this entirely, the ſpaces are ſo ſmall that a ſtraight fibre could not have given a ſufficient play, hence there is an obliquity. Further, if there had been only one row of oblique fibres, the ribs would have been moved either too much forwards or backwards, the motion would not have been equable; but by throwing in two rows, the extent of the motion is preſerved, and the motion is perfectly equable, the ribs moving parallel to each other; and as the upper ribs are more fixed than the under, when the intercoſtal muſcles are in action, there is a general motion upwards, the under ribs move towards the upper, and from top to bottom they move in this uniform manner; ſo that Dr BOERHAAVE was miſtaken in ſuppoſing that the under ribs were pulled downwards, while the motion of the upper ribs was directed upwards. They all move in the ſame direction.

AT the anterior parts the external intercoſtals are wanting, and the internal are wanting behind, and the reaſon of this is perfectly ſimple.

IF the external had been continued to the ſternum, the laſt fibres would have been fixed to a leſs moveable bone below, and to a rib above; therefore, inſtead of aſſiſting in raiſing the ribs, they would have pulled them down, and hence as ſoon as the intercoſtals approach ſo near, that from this obliquity the fibre would have been in danger of influencing the upper rib more than the under, they are laid aſide, they are carried no farther.

In the internal intercoſtal muſcles, that proportion is reverſed. In conſequence of their direction and the angle which the ribs make with the vertebræ, they would have come at length to influence the upper rib more than the under, when they would not only be uſeleſs, but even hurtful; and from this view, we are enabled to refute an error that has crept in, that the external and internal intercoſtals are antagoniſts to each other, that the external elevate the ribs, while the internal depreſs them. Whereas it appears from this, that they are intended to co-operate; and, without uſing further arguments, I have ſeen the co-operation in living animals, and it has been long ago very juſtly deſcribed by Dr Haller.

Atteſted by

Benj. Bell.

EXTRACT from the MINUTES of the PHYSICAL CLASS of the ROYAL SOCIETY of Edinburgh.

Edinburgh, 7th January 1793.

Dr MONRO, after having read a paper, entitled, *Obſervations on the Muſcles, and particularly on their Oblique Fibres*, requeſted of the Society that a committee might be appointed to examine into the dates of certain diſcoveries or improvements contained in the above paper. The Society accordingly appointed the following members, as a committee for that purpoſe, *viz.* Dr GREGORY, Dr RUTHERFORD, Dr DUNCAN, Dr THOMAS SPENS, Mr BENJAMIN BELL, Mr JAMES RUSSELL, Mr Profeſſor STEWART, Mr Profeſſor GREENFIELD, Mr Profeſſor PLAYFAIR. And the Society farther inſtructed this committee to examine into the dates above mentioned, and to take ſuch meaſures for aſcertaining them as they ſhould judge proper, and to report to the Society.

Extracted from the Minutes of the Royal Society,

By JOHN PLAYFAIR, *Sec. Phyſ. Clk.*

REPORT, &c. of the COMMITTEE.

Edinburgh,

The Committee referred to in the above extract met, and had laid before them, by Dr MONRO, a manuſcript, being an appendix to his paper, read in the Royal Society, January 7. 1793, in which were contained the following pieces:

A LITERAL copy from original notes of a Lecture in 1760, on Reſpiration, p. 26, 27. App.

FRAGMENT of a letter from Dr HAYGARTH, Phyſician in Cheſter, to Dr MONRO *junior*.

ANOTHER letter from Dr HAYGARTH to Dr MONRO, dated January 12. 1793.

ALSO paſſages copied from a manuſcript of Dr MONRO's Lectures, by THOMAS THORBURN, the copy of which is dated April 29. 1774. The paſſages being from the manuſcript, Vol. II. p. 348. 437. 466. &c.

ALSO from the manuſcript of the ſame Mr THORBURN, copied by his ſon Mr JOHN THORBURN, Lect. 27.—37.—40.

THE Committee having compared all the preceding extracts with the original papers, found them faithfully and correctly copied from them. In evidence of which they have ſigned this atteſtation.

D. RUTHERFORD.

J. GREGORY.

AND. DUNCAN.

WM. GREENFIELD.

JAMES RUSSELL.

BENJ. BELL.

TH. SPENS.

DUGALD STEWART.

I SHALL next ſubjoin a letter I wrote to the following gentlemen, with their anſwers *.

Mr *Dugald Stewart*, formerly Profeſſor of Mathematics, and at preſent Profeſſor of Moral Philoſophy in the Univerſity of Edinburgh.

Dr *Daniel Rutherford*, Profeſſor of Botany in Edinburgh.

Dr *Andrew Duncan*, Profeſſor of the Inſtitutions of Medicine in Edinburgh.

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* Dr *Stewart* attended my Courſe of Lectures in 1770.

Dr *Rutherford* in 1764, 1765, 1766, 1767.

Dr *Duncan* in 1764, 1766.

Dr *Gregory* in 1768, 1771, 1772.

Mr *Bell* in 1766, 1767, 1768, 1770.

Mr *Ruſſell* in 1771, 1772, 1774, 1775.

Dr *James Home* in 1777, 1778, 1779.

Dr *Hope* in 1783, 1784, 1785, 1786.

Mr *Greenfield* in 1790.

Dr *James Gregory*, Profeſſor of the Practice of Phyſic in Edinburgh.

Mr *Benjamin Bell* and Mr *James Ruſſell*, Surgeons in Edinburgh.

Dr *James Home*, Phyſician in Edinburgh.

Dr *Thomas Hope*, Profeſſor of Medicine in the Univerſity of Glaſgow.

The Reverend Mr *William Greenfield*, Profeſſor of Rhetoric and Belles Lettres in the Univerſity of Edinburgh, and at preſent conjoined with Profeſſor *Robiſon* in teaching the Claſs of Natural Philoſophy.

SIR,

As I intend to publiſh very ſoon ſome obſervations on the muſcles, and wiſh to aſcertain the time at which I firſt demonſtrated, that a greater extent of motion can be produced by oblique than by ſtraight muſcles, I hope you will excuſe me for aſking the favour of you to anſwer the following queſtions:

1. Do you recollect, that when you did me the honour of attending my Lectures, I endeavoured to demonſtrate by mathematical figures drawn with chalk upon a board, and by arithmetical calculations, that oblique muſcles perform more extenſive motions than ſtraight muſcles contracted in the ſame proportion?

2. Do you recollect the particular diagrams or calculations I then employed?

3. Do you recollect that I diſſected the two layers of the intercoſtal muſcles in ſuch a manner as to reſemble the mathematical figures I had drawn with chalk on the board, and that I applied my demonſtrations not only to the intercoſtal muſcles, but to muſcles in general, and particularly to the abdominal muſcles, the muſcles of the back part of the head and trunk of the body, and to the penniform muſcles of the extremities, when theſe ſeveral claſſes of muſcles were demonſtrated?

4. Do you recollect, that when I explained the action of the abdominal muſcles, and compared the action of the oblique muſcles with that of the recti, I obſerved, that if the lower ends of the recti had been fixed to the oſſa ilia, inſtead of the oſſa pubis, they would have made a more extenſive motion of the cartilago enſiformis, or would have drawn it

lower down or nearer to the oſſa pubis, than is poſſible for them to do in their real ſituation?

I am, Sir,

Your moſt humble ſervant,

ALEX. MONRO.

Edinburgh, February 11. 1793.

To this Letter I received the following Anſwers.

DEAR SIR,

I was ſo very young when I attended your Lectures, and my attention has been ſo much occupied ſince that time with other ſtudies, that, notwithſtanding the great pleaſure with which I heard them, I retain a very indiſtinct remembrance of them at preſent. I am not able to ſay any thing ſatisfactory in anſwer to the queries you propoſe. But I recollect, in general, that you remarked, the greater extent of motion produced by oblique than by ſtraight muſcles contracted in the ſame proportion; an obſervation which might probably have eſcaped my memory, with many other parts of your Courſe, if it had not led you to take notice of ſome very beautiful circumſtances in the mechaniſm of the body. I recollect likewiſe, that you illuſtrated your ideas on the ſubject by means of a diagram; but at this diſtance of time, I have perfectly forgotten your mode of demonſtration.

I am, Dear Sir,

Your moſt obedient humble ſervant,

DUGALD STEWART.

Argyle Square, 17*th February* 1793.

DEAR SIR,

I received the letter addreſſed to me, in which you propoſe ſome queſtions reſpecting the doctrine you delivered of the action of oblique muſcles, and the advantages obtained by the oblique direction of their fibres, when I had the honour of attending your Anatomical Lectures. So many years have elapſed ſince that time, that many particulars relating to what then paſſed have ſlipped from my memory, and I derive but little aſſiſtance, upon the preſent occaſion, from the Notes of your Lectures I have preſerved, they happen to be ſo conciſe on the point in queſtion. What I do clearly recal to remembrance, I ſhall freely communicate to you. I recollect perfectly, that you took great pains to demonſtrate the direction of the fibres, and alſo to point out the extent of both the external and internal intercoſtal muſcles; and that afterwards, when treating of the action of theſe muſcles, you obſerved, that not only both of them ſerved to elevate the ribs, but alſo that, by reaſon of the obliquity of their fibres, they muſt cauſe a much greater extent of motion of the ribs, by the ſame proportional contraction, than they could have done had their fibres run directly and perpendicularly from one rib to another.

The diagrams and calculations by which you eſtabliſhed the latter obſervation, I do not preciſely recollect; to me, however, I know they were quite ſatisfactory. One very ſimple illuſtration I well remember, to wit, your placing two blow-pipes betwixt two parallel lines, ſo that their points meeting together, formed an angle at one of the parallels, while the other ends reached to and were kept conſtantly at the ſame diſtance from each other on the other parallel; then gradually approaching the points of this ſecond parallel till they coincided with it. By the ſucceſſive points of interſection of the blow-pipes in this progreſs, you remarked might be denoted the gradual contraction of oblique muſcular fibres, repreſented by the blow-pipes, and the motion of any part attached to them correſponding to this contraction; and by the diſtance betwixt the points of the blow-pipes, as theſe overlapped each other when coinciding with the parallel, was denoted twice the quantity by which ſuch fibres muſt have contracted, in order to bring the two parallels together, or to bring bones ſituated like them into contact, while a fibre placed perpendicularly betwixt them, to effect the ſame extent of motion, muſt be ſuppoſed to have contracted by a quantity equal to its whole length, or to have been reduced to a mere point. This illuſtration appeared to me ſo clear and deciſive, that nothing could add to the conviction it conveyed; I was then much more accuſtomed to geometrical than to anatomical demonſtrations, and it at once recalled to me ſome of the known properties of triangles, and particularly the relative diminution of the ſecant and tangent, as an angle is diminiſhed. I was ſo delighted with the beautiful mechaniſm of the parts I had ſeen, and with the ingenious explanation you had given of their action, that I well recollect I ſoon thereafter made a ſmall model to imitate their motion. It conſiſted of an upright pillar, from which were extended two horizontal arms, one over the other, the lowermoſt of which was moveable. Threads were attached to this arm, and *were paſſed through ſmall rings on the uppermoſt arm*, having ſuch obliquity as the fibres of the different layers of the intercoſtal muſcles; by pulling or ſhortening the threads, the lowermoſt arm was drawn upwards, preciſely as the ribs are by the contraction of the intercoſtal muſcles.

I DO not recollect to what other particular muſcles you extended the doctrine above mentioned, but I underſtood, that you conſidered it as general, and conſequently applicable more or leſs ſtrictly to every oblique muſcle whatever.

I am, with much regard,

Dear Sir,

Your moſt humble ſervant,

D. RUTHERFORD.

Edinburgh, February 14. 1793.

DEAR SIR,

I received your's yeſterday, and am very ſorry that my recollection does not enable me to give any poſitive or preciſe anſwer to your queſtions. I attended your Lectures during the winter ſeſſions 1764–5, and 1766–7. Ever ſince that period I have been ſatisfied, that oblique muſcles perform more extenſive motions than ſtraight ones contracted in the ſame proportion. But I do not particularly recollect the demonſtration you gave of it at that time. Nor do I find any account of it in the ſhort notes which I took from your Lectures. In theſe, however, I find the following paragraph: "HALLER "imagines, that the reaſon why the intercoſtals are placed "obliquely is, that there may be room for the inſertion of a "greater number of muſcular fibres; but when we reflect, "that had they been placed perpendicularly, the ribs could "not have been brought by far ſo near one another, we ſee "a much more ſatisfactory reaſon for it."

I remain your's ſincerely,

ANDREW DUNCAN.

Edinburgh, 13*th February* 1793.

St John's Street, Wedneſday Evening, 13*th February* 1793.

Dear Sir,

I am very ſorry that I have neither any notes of your Lectures when I was your pupil, nor yet ſuch particular remembrance of your doctrine about the purpoſe and the advantage of oblique muſcles, as to enable me to anſwer, in a ſatisfactory manner, the queries that you have put to me.

I remember perfectly the general principle, and the application and illuſtration of it in the caſe of the two layers of intercoſtal muſcles; but the more full illuſtration of it by diagrams, arithmetical calculations, and geometrical reaſoning, ſuch as I had the pleaſure of hearing from you at the Royal Society laſt month, had eſcaped my memory. Perhaps I had attended to them leſs than I ſhould have done, and forgot them the more readily, becauſe the fundamental propoſition is ſo nearly ſelf-evident, that the very ſlighteſt illuſtration, or even a clear and preciſe enunciation of it, ſuch as I am ſure you would not fail to give, muſt have convinced me at once of its truth. Obſerve, I attended your Lectures three different years (if I remember right) between 1768 and 1773, both incluſive.

Your's moſt truly,

J. Gregory.

Mr Benjamin Bell, in anſwer to my firſt queſtion, anſwers, That he does; but in anſwer to my three queſtions, ſays he does not recollect.

Dear Sir, *Edinburgh*, 22*d February* 1793.

I was lately favoured with a letter from you, requeſting me to recollect the account you gave in your Lectures, when I had the honour of attending them, of the advantages procured by the oblique direction of muſcular fibres. The ſubject I well remember was a very favourite one with you, and that you inſiſted at great length upon the explanation of certain doctrines which you conſidered as peculiar to yourſelf. I remember too, that you endeavoured to illuſtrate your opinions by the delineation of ſome figures upon a board. But I cannot, at this diſtance of time, charge my memory with the diſtinct recollection of what theſe figures were, neither can I be poſitive with regard to the nature of theſe peculiar doctrines. I recollect, however, that certain doubts occurred to me at the time, which I ſuggeſted to you in converſation, and which, I think, you then reſolved to my ſatisfaction.

Of the ſecond and third queries I remember nothing diſtinctly.

Respecting the fourth query likewiſe, my recollection is not very accurate, though I have ſome faint remembrance of your attempting to prove that the recti muſcles, if their lower end, in place of being inſerted in the os pubis, had been inſerted into the ſpines of the oſſa ilia, would have had more effect in drawing the ſternum down to the pelvis.

These are all the circumſtances I now recollect about the ſubjects ſtated in your queries to me; and although the anſwer be deficient in many reſpects, I am yet confident, that, ſo far as they go, they give a fair ſtatement of the ſubſtance of your Lectures.

I am,

Dear Sir,

Your moſt obedient and humble ſervant,

James Russell.

DEAR SIR, *Edinburgh, April* 23. 1793.

I RECEIVED your letter ſome time ago, and would have anſwered your queries ſooner, but I wiſhed to conſult the notes which I took of your Lectures. Theſe I have not as yet been able to find. What I am, therefore, about to ſtate is from memory.

WHEN I had the pleaſure of attending your Lectures in the years 1777, 1778, 1779, while treating of the motions of muſcles, you ſtated the common idea of the advantage of oblique muſcles over ſtraight muſcles, *viz.* that by the obliquity of their direction, room was given for the inſertion of a greater number of muſcular fibres, and that this increaſed number of fibres more than counterbalanced the power loſt by the obliquity of their direction. But you obſerved, that there was ſtill a further advantage derived from the obliquity of muſcular fibres: that, when two muſcles were placed betwixt two parallel lines, the fibres of one of which were oblique, and thoſe of the other perpendicular to theſe two parallel lines, and both of theſe muſcles contracting in the ſame proportion to their lengths, that then the quantity of motion performed by the former was greater than that performed by the latter: Or, that the quantity of motion performed by theſe two kinds of muſcles being equal, the degree of contraction of the oblique muſcle was leſs than that of the ſtraight muſcle.

YOU illuſtrated the above propoſition by geometrical figures and a ſimple arithmetical calculation. But, at this diſtance of time, it is impoſſible for me to recollect the particular diagrams or arithmetical figures.

You applied this general theorem to explain the action of the oblique muſcles over the whole body; but more eſpecially, you called the attention of your hearers to it, when you treated of the muſcles of the abdomen and thorax.

With regard to the muſcles of the abdomen, you compared the action of the obliquus externus and internus with that of the rectus, and ſhewed that the two firſt acting together, performed in the diagonal a motion greater than was actually performed by the rectus.

When treating of reſpiration, you agreed with Haller and other phyſiologiſts, that the action of the external and internal intercoſtal muſcles was not antagoniſtical, as had been ſuppoſed by many, the one being employed in elevating, and the other in depreſſing the ribs; but that both of them, acting at the ſame time, elevated the ribs, and were therefore muſcles of inſpiration. Beſides this, you delivered as an idea peculiar to yourſelf, that by the obliquity of the fibres of the intercoſtal muſcles, a much greater quantity of motion was performed, than if perpendicular fibres had been uſed between the ſame diſtance of the ribs. Conſequently, if their action had been employed, a greater length of muſcle muſt have been neceſſary to perform the ſame quantity of motion; and thus a leſs quantity of bony ſurface would have been preſented as a ſhield to defend from injuries the important organs which the thorax contains. This you explained, by diſſecting the intercoſtal muſcles, and preſenting different views of them. You meaſured them, and the diſtance of the ribs, and applying the above propoſition to the actual meaſurement, you ſhewed the advantage of the preſent ſtructure. And laſtly, you thought, that by employing two oblique muſcles inſtead of one, the ribs were more directly elevated than they would have been by one ſet of oblique fibres.

WHETHER you applied the above propoſition to explain the action and advantages of the penniform muſcles, or of the muſcles on the back of the head and of the trunk of the body, I cannot ſo poſitively aſſert; but I rather think that you did, at leaſt the idea was not new to me when I lately heard it applied to theſe particular muſcles. I am confident, however, that I cannot be miſtaken in what I have ſtated above, as ideas delivered in your Courſe of Lectures, when I had the honour of attending them, ſince at that time they made a ſtrong impreſſion on me, as a moſt beautiful piece of mechaniſm in the animal body, by which the greateſt quantity of motion was performed by the leaſt power.

I have the honour to be,

SIR,

Your moſt obedient humble ſervant,

JAMES HOME.

SIR,

A FEW days ago I had the honour of receiving your letter, containing ſeveral queries. In reply, I have to acquaint you, that I recollect, that, in your Lectures, you demonſtrated by mathematical figures, drawn with chalk upon a board, that oblique muſcles perform more extenſive motions than ſtraight ones contracted in the ſame proportion; and that you employed arithmetical calculations to eſtimate the difference.

To the beſt of my recollection, the diagram you figured was an iſoſceles triangle, having a line drawn from the apex perpendicular to the baſe. In this figure two ſides of the triangle repreſented oblique muſcles, and the perpendicular drawn from the angle formed by them repreſented a ſtraight one. At ſame time, you ſhowed, by dividing the two ſides and perpendicular into ſo many equal portions, and drawing lines from the angles at the baſe to the perpendicular, that, a decurtation of oblique muſcles, ſuppoſe of one-fifth part of their length, would cauſe the apex, which is to be conſidered as a moveable point, to approach nearer to the baſe, in the direction of their diagonal, than would be occaſioned by a proportional contraction, that is in the preſent ſuppoſition, by a contraction of one-fifth of the length of a ſtraight muſcle. From my memory, as well as from the notes I took from ſome parts of your Lectures, I am certain of your application of theſe doctrines, to the oblique muſcles conſtituting the two layers of the intercoſtals. From the latter, I find, you computed that the intercoſtal muſcles, in conſequence of their obliquity, produce by their action, nearly three times as much motion of the ribs, as would take place, were they ſtraight and contracted in the ſame ratio. My memory does not enable me to ſpeak ſo preciſely and decidedly reſpecting the application of them, to the other oblique muſcles of the body. I have no notes from that part of your Courſe of Lectures in which you treated of their action.

I am,

SIR,

Your obedient ſervant,

THOMAS CHA. HOPE.

College, Glaſgow, February 1793.

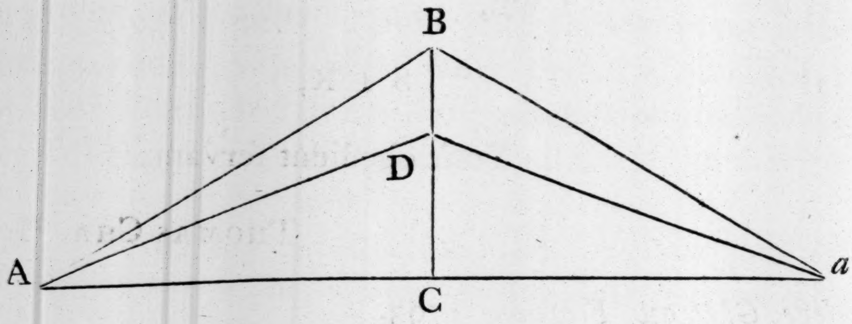
P. S. I MUST apologiſe to you for being ſo long in acknowledging and anſwering your letter. I aſſure you I have ſeized the firſt moment of leiſure for writing.

T. C. H.

SIR,

I AM favoured with your letter, in which you propoſe to me ſome queſtions reſpecting the account which you delivered in your Lectures of the beautiful fact in Phyſiology, that oblique muſcles perform more extenſive motions than ſtraight ones which are contracted in the ſame proportion.

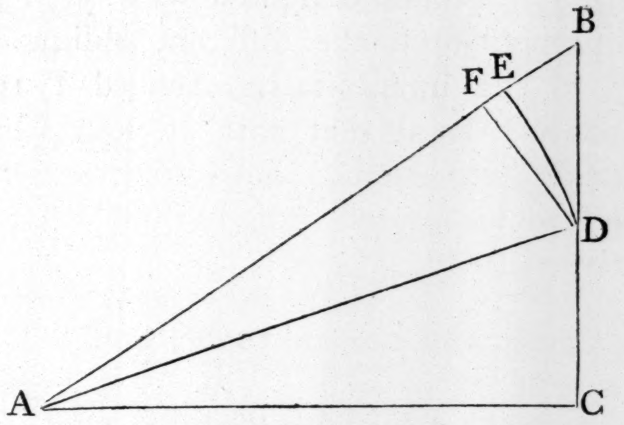
I REMEMBER in general, that you ſhewed the truth of the propoſition with great diſtinctneſs, both by means of geometrical figures, and of arithmetical calculations. To the beſt of my recollection, your account of it was to the following purpoſe:



AB (ſee Tab. 2. fig. 11.) repreſents a muſcle, the extremity A is conſidered as fixed; but the other extremity B, when the muſcle is contracted, moves in the ſtraight line BC, to which AB is oblique. From A the fixed extremity of the oblique muſcle, let AC be drawn perpendicular to BC; and let the other extremity by means of the contraction, be removed from B to D. In performing this motion, the oblique muſcle AB is not only leſs ſhortened in proportion to its length, than a ſtraight muſcle, ſuch as CB would have been, but it is in fact ſhortened by a ſmaller quantity than CB would have been; or the difference between AB and AD is leſs than BD, and even when another oblique muſcle, as *a* B, conſpires to produce the motion, the contractions of both the oblique muſcles taken together will not only be leſs in proportion to their lengths than the contraction of CB in proportion to its length; but farther, if AB and *a*B are each of them longer than twice CB, the contractions of both of them together will be leſs than BD. The figure you employed was, I believe, ſuch as

I have drawn; and if I am not miſtaken, you ſhewed the truth of the propoſition by different arithmetical calculations in the following manner: You firſt took three numbers repreſenting the hypothenuſe, and the ſides of the right angled triangle ABC; and then ſuppoſing the ſide BC to be diminiſhed by a certain quantity, you computed what would be the length of the hypothenuſe.

I CANNOT pretend to recollect the general demonſtration which you gave of the propoſition. The following one readily occurs, and may poſſibly be the ſame that I heard from you, though it is more probable that your's was a ſimpler and a better one.



ROUND A as a centre, (ſee Tab. 4. fig. 13.) with AD as a radius, deſcribe the arch DE meeting AB in E; and draw DF meeting AB at right angles in F. The contraction of the oblique muſcle is BE; this is always leſs than BF; BF is always leſs than BD; and therefore, *a fortiori*, BE is leſs than BD. But more particularly, the triangle BFD, ACB, are ſimilar; for the angles at C and F are right angles, and the angle ABC is common to both the triangles; therefore BD : BF : : AB : BC.

So that in performing the ſame motion the contraction of the ſtraight muſcle would exceed the contraction of the oblique, in a greater proportion than that in which the length of the oblique muſcle exceeds the length of the ſtraight one. So that if AB were double of CB, the contraction of the ſtraight muſcle would be more than double the contraction of the oblique. In very ſmall contractions, however, the contraction of the ſtraight muſcle would be to that of the oblique in the proportion of AB to BC *ſenſibly*; for if BE is very ſmall in compariſon of AB, BF and BE will be *ſenſibly* equal.

As my knowledge of phyſiology is extremely ſlight, I cannot pretend to anſwer the remaining queſtions. I remember, however, that I was much delighted with your application of the general propoſition to the different oblique muſcles, and particularly to the intercoſtals. Indeed I received very great ſatisfaction from all that part of your Courſe which I was ſo fortunate as to attend; and I often regretted that I had not more leiſure to impreſs your valuable Lectures on my mind by private ſtudy.

I have the honour to be, Sir,

Your moſt obedient humble ſervant,

WM. GREENFIELD.

Caſtlehill,
Saturday, 19th February 1793.

Edinburgh, October 31. 1793.

THIS evening, after the preſs was ſo far advanced, I received from Mr JOHN BELL, ſurgeon in Edinburgh, a work he, this day, publiſhed on the Anatomy of the Bones, Muſcles and Joints, in which, page 289. he has, with much candour, done me juſtice on the ſubject in queſtion, by printing the following note:

"I REMEMBER many years ago, to have heard Dr MONRO
" explain the office of the intercoſtal muſcles by a diagram,
" deducing from that argument, the more powerful effect of
" all muſcles having oblique fibres."

Mr JOHN BELL attended my Lectures in the years 1775 and 1776.

ALEX. MONRO.

I SHALL now conclude with the few following remarks:

1. Dr BLANE now ſays, That there can be no doubt he was led to conſider this ſubject from that part of my Courſe which related to the muſcles, and particularly the intercoſtals. Why then did he not acknowledge this in his Croonian Lecture?

2. Dr Blane next ventures to aſſert, that during his attendance, he (Dr Monro) never made uſe of any geometrical demonſtration, but only ſuch groſs illuſtrations, as were ſuited to his mixed audience. On this I would make the following obſervations: The *firſt* is, That from the facts already proved, it appears, that I demonſtrated this matter, firſt geometrically, founding on the propoſitions of Euclid; and, then, arithmetically, in order to make my demonſtration more convincing: 2*d*, Dr Blane has thought proper to uſe the terms Groſs Illuſtrations, of which it is not in his power to give the ſpecific meaning: becauſe, nothing but geometrical demonſtration could have led me to perceive the truth of this propoſition, or enabled me to prove it to others. And, it appears, that from theſe illuſtrations, Dr Blane was ſo much convinced of the truth of the doctrine, as to try whether he could hit upon ſome other mode of proving its truth.

3. Dr Blane affirms, that my (Dr Monro's) obſervations were confined to the intercoſtal muſcles, which the Reader has found to be contrary to fact; for the application of this doctrine was made when I treated of the abdominal muſcles; the intercoſtal muſcles; the muſcles of the whole trunk; the penniform muſcles of the extremities; particular muſcles; and ſo often that there was more danger of my tiring the ſtudents with it, than of not inculcating it ſufficiently. How all this ſhould have eſcaped Dr Blane, during his two years attendance, is truly ſurpriſing; eſpecially as he tells us, that he was, at the time, led to conſider this ſubject from that part of my Courſe which related to the muſcles.

4. To ſupport his pretenſions to the firſt diſcovery of a Geometrical Demonſtration, he tells us how well his demon-

ſtration was received in a club of ſtudents. Some Readers will, perhaps, think it ſufficient for me to obſerve upon this, that theſe young gentlemen muſt have been as inattentive as himſelf: but the real fact may have been, that although Dr Blane copied very nearly the figure I always employed in my demonſtration; yet, by adding to it a number of A^s and B^s and C^s, affecting the ſtyle of Euclid, he gave it the appearance of originality. But I appeal to the Reader, whether my demonſtration is not more ſimple and intelligible to a beginner than his is, and equally convincing to a mathematician.

5. Not contented with aſſerting what has been diſproved, that my obſervations were entirely confined to the intercoſtal muſcles, he adds, the mechaniſm of which he has made uſe of, rather as an exception, than illuſtration of his doctrine.

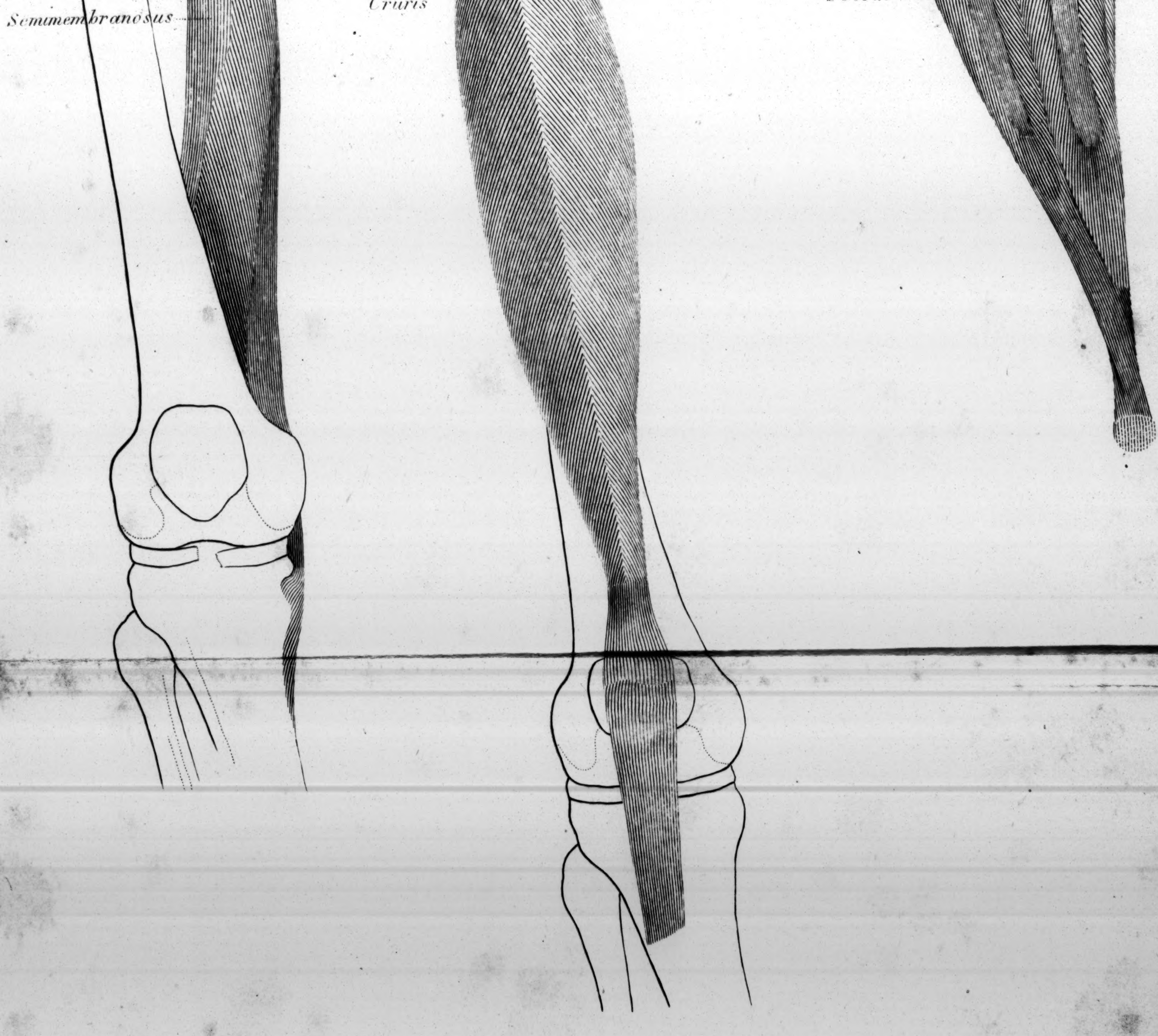
If Dr Blane could have made good the propriety of this exception, and at the ſame time perſuaded his reader that my groſs illuſtrations, or even demonſtrations, were applied to the intercoſtal muſcles only, that is, were miſapplied; his remarks would have been, as he phraſes it, wholly his own.

But, on this point, the Doctor has been ſingularly unfortunate: for, as the number of the fibres of the intercoſtal muſcles or their ſtrength is leſſened, inſtead of being increaſed, by their obliquity; it is evident that their obliquity could ſerve the purpoſe only of rendering the motion of the ribs more extenſive than could have been performed by ſtraight or perpendicular fibres.

6. Dr **Blane** ſays, if it ſhould be alleged that his remarks are only an extenſion or improvement of what was delivered in my Lectures, he is ready to allow this.

But I conclude by obſerving, that Dr **Blane**'s modification of my Demonſtrations is ſo far from being an extenſion or improvement of them, that it only ſhows that oblique muſcles can perform a more extenſive motion than ſtraight ones placed between the ſame parallels; whereas I proved, not only that oblique muſcles can do ſo, but that, with the ſame degree of contraction, they can perform a more extenſive motion than ſtraight muſcles of equal length.

FINIS.


Semimembranosus
Cruris

Tab. 1.

Fig. 1.

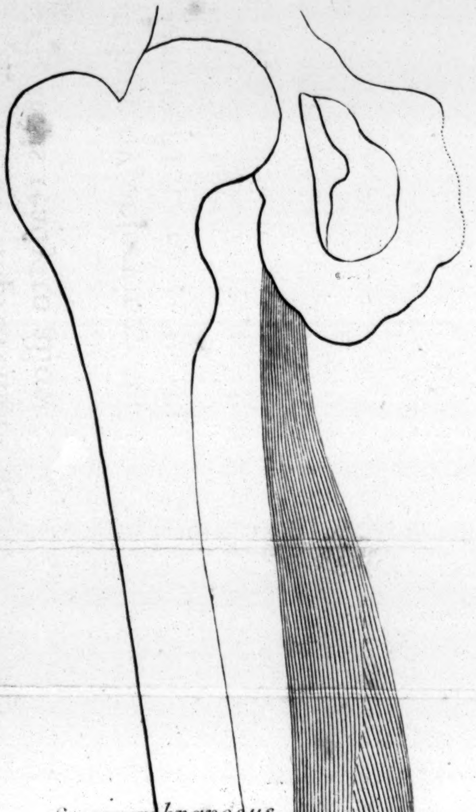


Fig. 2.

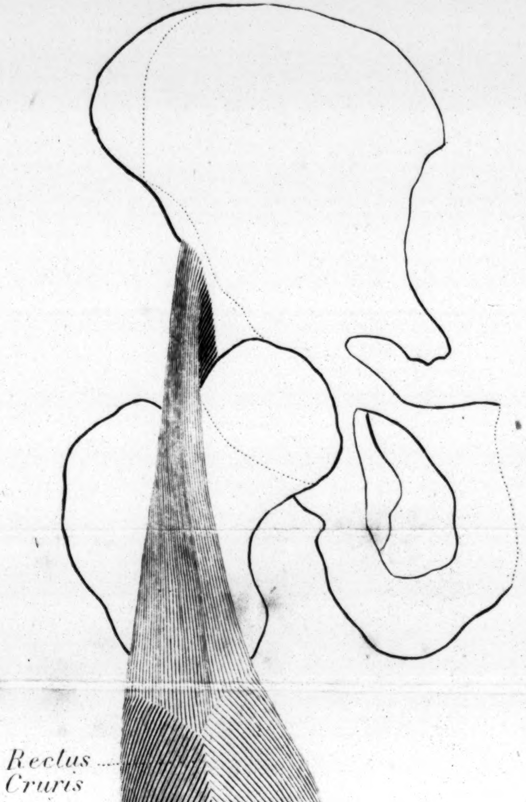
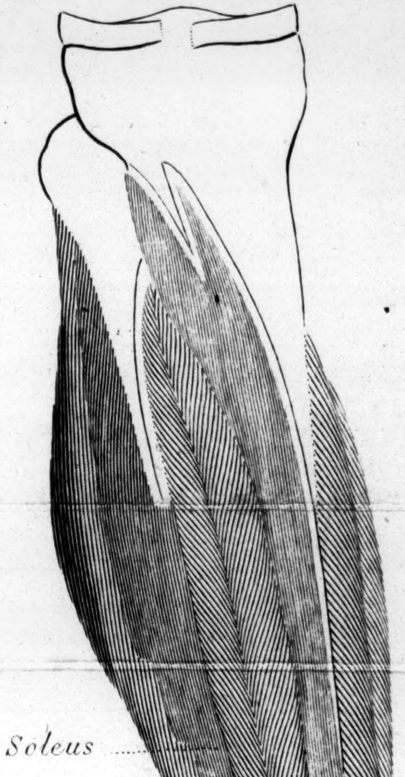


Fig. 3.



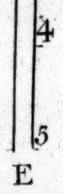


Fig. VII.

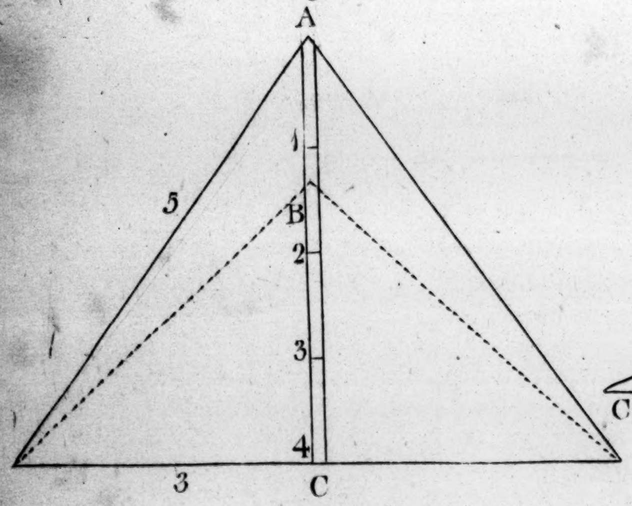


Fig. VIII.

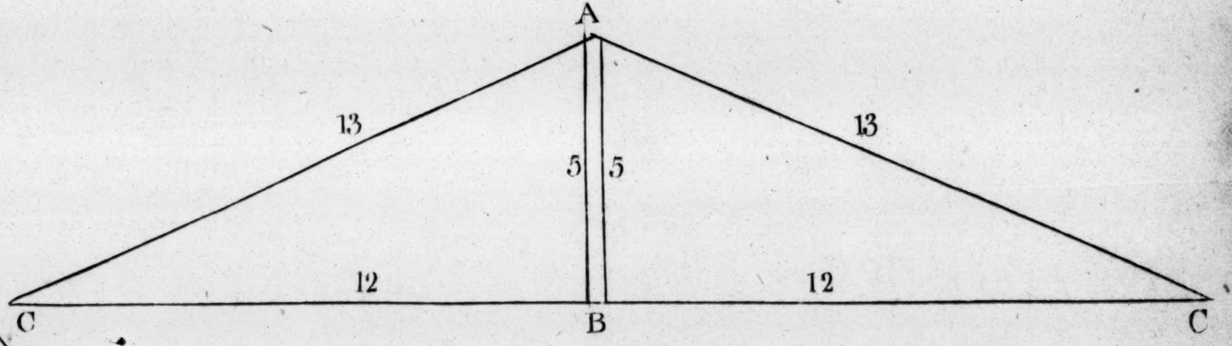


Fig. IX.

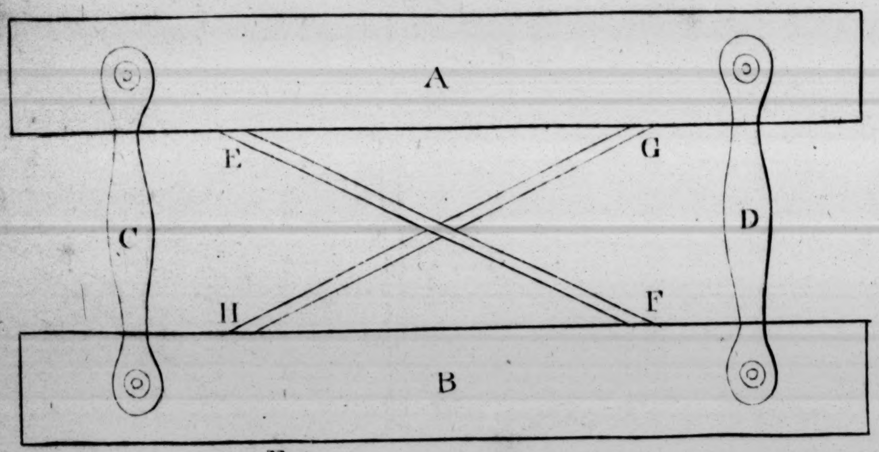
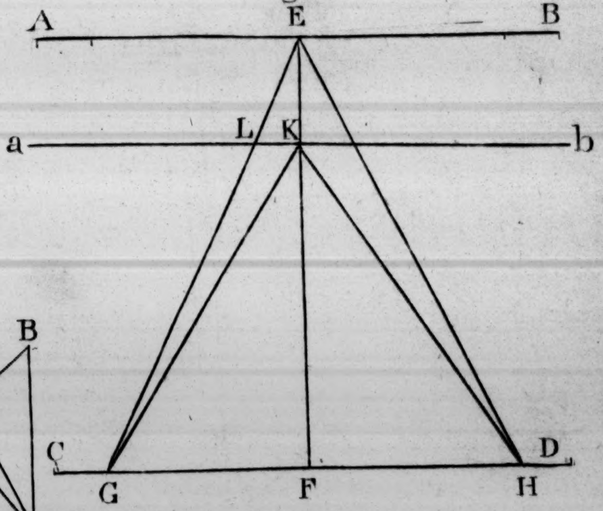


Fig. X.



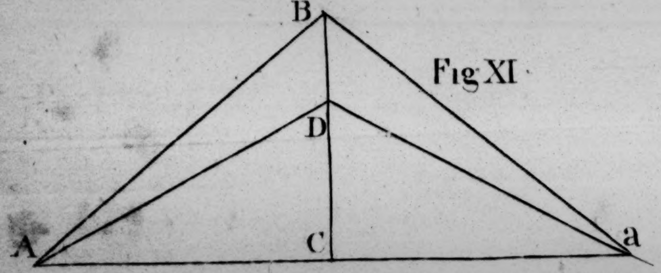


Fig. XI

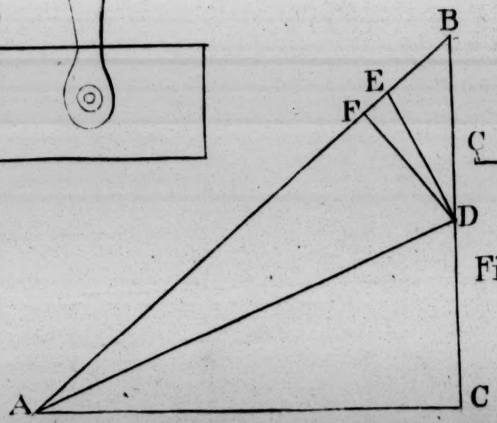


Fig. XII

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TAB. II

Fig. I

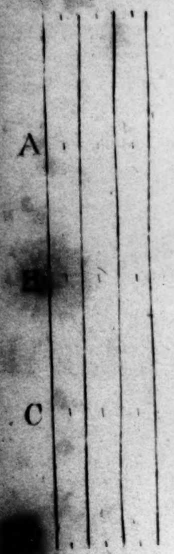
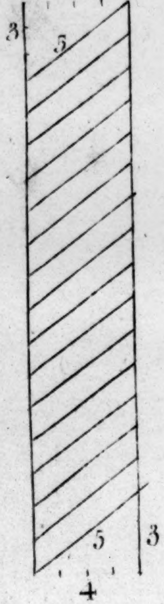


Fig. II



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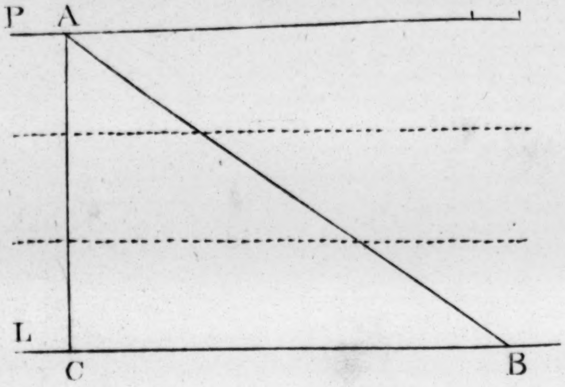

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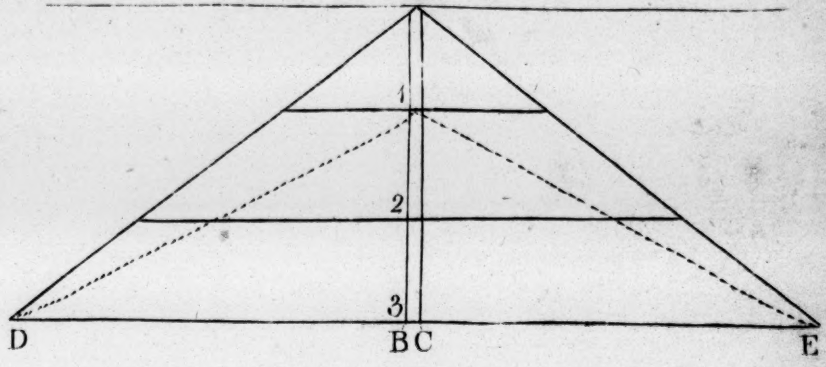

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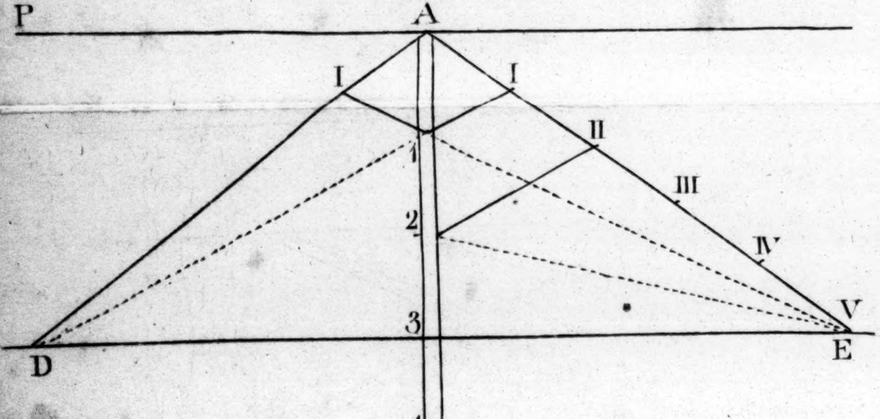

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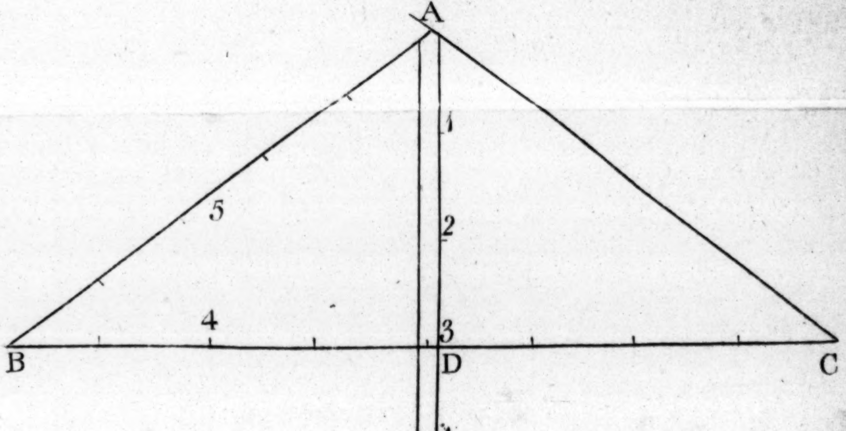
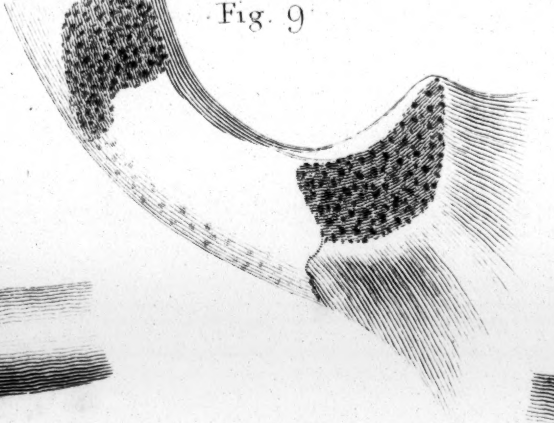
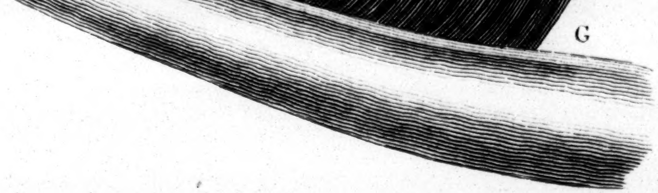


Fig. 9





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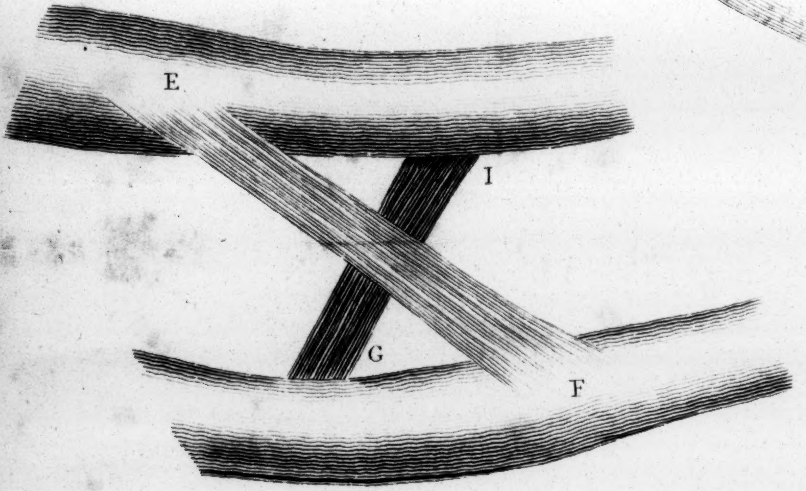

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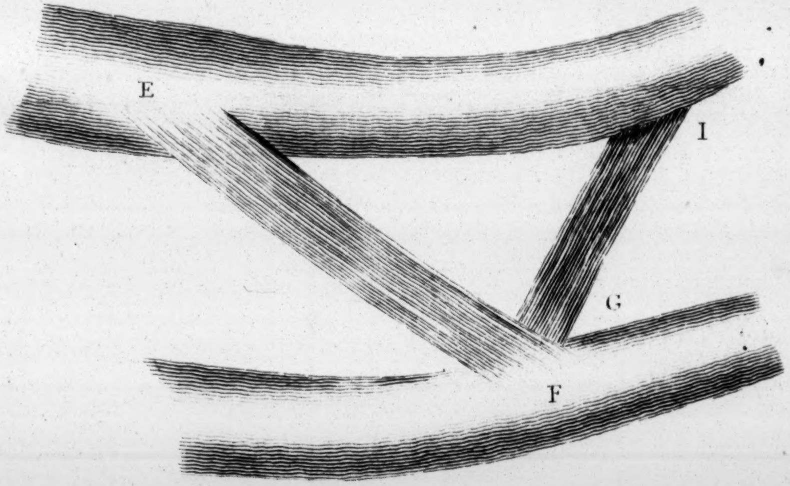

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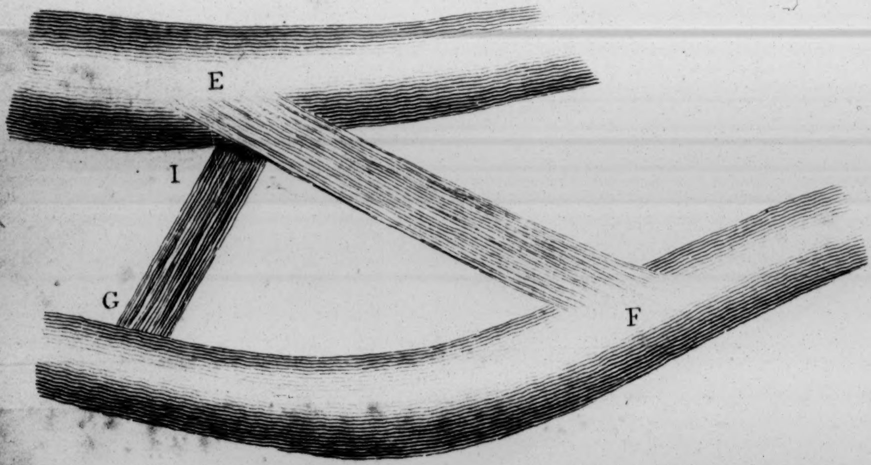

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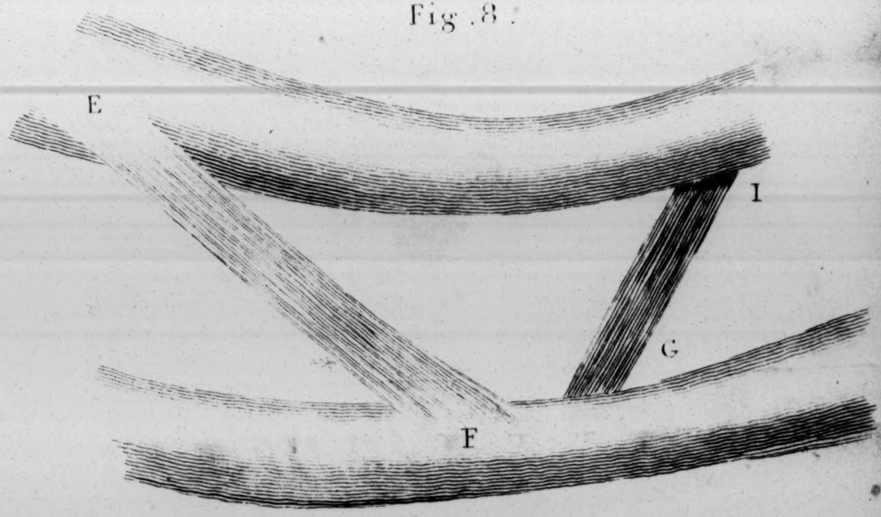


A. Fyfe del^t.

Tab. III.

Fig. 1.

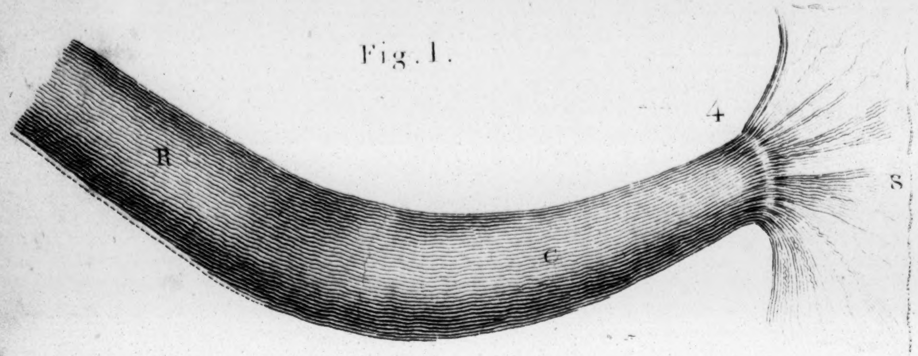


Fig. 2.

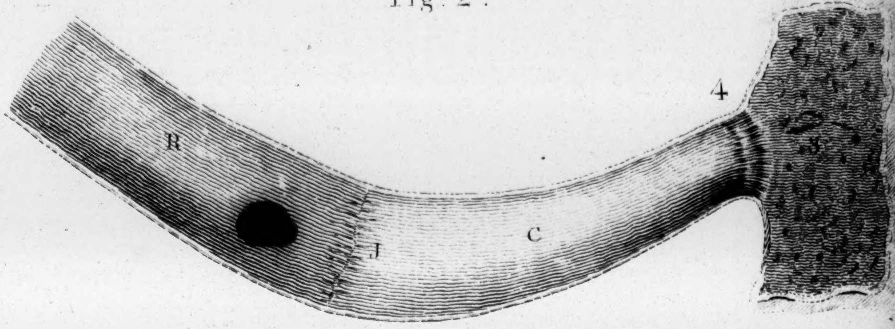
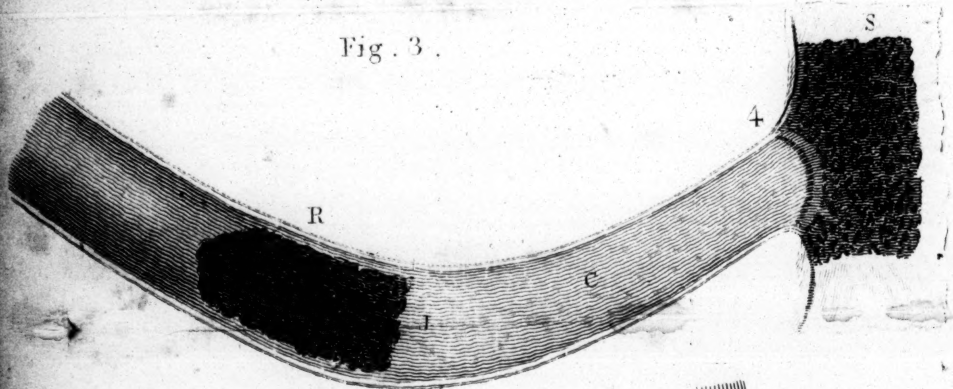


Fig. 3.



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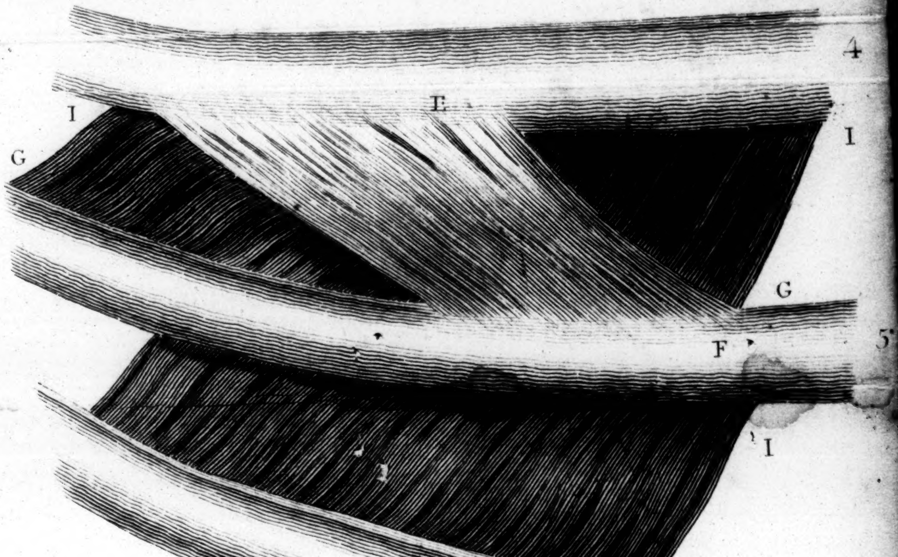

Fig. 9

TAB. IV